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ART. XXIX.—*On some controverted points in Geological Climatology; a reply to Professor Newcomb, Mr. Hill and others;* by JAMES CROLL, LL.D., F.R.S.

NINETEEN years ago the theory was advanced that the Glacial epoch was the result of a combination of physical agents brought into operation by an increase in the eccentricity of the earth's orbit. Few or no objections have been urged against what may be called the astronomical part of the theory. But the portions relating to these physical agencies, which is by far the most important part, have from time to time met with considerable opposition. Considering the newness of the subject, and the complex nature of many of these combinations of physical agencies, it would not be surprising if some of the original deductions in regard to them proved erroneous. But after long and careful re-consideration of the whole matter I have not found reason to abandon any of them or alter them to any material extent.

The only class of objections urged against the theory which I have as yet considered at length are those relating to the cause of ocean currents, and their influence on the distribution of heat over the globe; and I think, it will be admitted that the views which I have advocated on these points are now being pretty generally accepted.

But it is in reference to the influence of aqueous vapor, fogs and clouds on the production and preservation of snow that the greatest diversity of opinion has prevailed. The object of the present article is to examine at some length the principal

objections which have been advanced in regard to this part of the inquiry. I shall also take the present opportunity of discussing more fully some points on which I have been sometimes misunderstood, and which appear to have been treated rather too briefly on former occasions.

In the American Journal of Science for April, 1876, Professor Newcomb has done me the honor to review at some length my work, 'Climate and Time.' And as his article is mainly devoted to a criticism of my reasoning in regard to those very points to which I refer, I shall begin with an examination of his objections. One reason for entering at some length into an examination of Professor Newcomb's objections is the fact that they embrace to a large extent those which have been urged by reviewers in Great Britain. Some of his objections, however, as will be seen, are based upon a misapprehension of my reasoning.

Temperature of Space.—One of the most important factors in the theory of geological climate resulting from changes in the eccentricity of the earth's orbit is obviously the temperature of stellar space. Unless we have, at least, some rough idea of the proportion which the heat derived from the stars bears to that derived from the sun, we can not form any estimate of how much the temperature of our earth would be lowered or raised by a given decrease or increase of the sun's distance.

The question of the temperature of space has been investigated in different ways by Pouillet and Herschel; and the result arrived at was that space has a temperature of $-239°$ F., or an absolute temperature of 222°. The mean absolute temperature of our earth is about 521°. Consequently, according to these results, the heat received from the stars is to that received from the sun as 222 to 299. All my determinations of the change of temperature due to changes in the sun's distance were computed on these data, although I believe, for reasons stated, that space must have a much lower temperature. Recent observations of Professor Langley made during the Mount Whitney Expedition confirm the correctness of my belief.

Professor Newcomb, however, wholly ignores all that has been done on that subject, for he commences his review by the statement that "practically there is but one source from which the surface of the earth receives heat—the sun, since the quantity received from all other sources is quite insignificant in comparison."

Surely Professor Newcomb must have forgotten all about the researches of Pouillet and Herschel into what has been

termed the 'Temperature of Space,' or he could not have affirmed so positively that "practically there is but one source from which the earth receives heat, and that all other sources are quite insignificant" without, at least, giving some reason for the assertion.

I am pleased to find that he agrees, in the main, with what has been advanced in 'Climate and Time,' in reference to the heating power of ocean currents, and also as to their existence being due to the impulse of the winds. But he differs widely from me in regard to the heat conveyed by aerial currents.

On the Heat conveyed by Aerial Currents.—I stated that the quantity of heat conveyed from equatorial to high temperate and polar regions is trifling in comparison with that conveyed by ocean currents. For the heated air rising off the hot ground of the equator, after ascending a few miles becomes exposed to the intense cold of the upper regions, and having to travel polewards for thousands of miles in those regions, it loses nearly all the heat which it brought from the equator before it can possibly reach high latitudes. To this Professor Newcomb objects as follows: "He (Mr. Croll), speaks of the hot air rising from the earth and becoming exposed to the intense cold of the upper regions of the atmosphere. But what can this cold be *but the coldness of the very air itself which has been rising up?* If the warm air rises up into the cold air, and becomes *cooled by contact* with the latter, the latter must become warm by the very heat which the former loses, and if there is a continuous rising current the whole region must take the natural temperature of the rising air. This temperature is, indeed, much below that which maintains at the surface *for the simple reason that air becomes cold by expansion* according to a definite and well-known law. Having thus got his rising current constantly cooled off *by contact with the cold air* of the upper regions, it has to pass on its journey towards the poles," etc., p. 267.*

Here the cooling of the ascending air is attributed to two causes (1) the heat lost by expansion as the air rises; (2) the heat lost by contact with the colder air through which the ascending air passes and with which it mixes in the upper regions. But the two may be resolved into one, viz: the heat lost by expansion; for the cold air to which the ascending air communicates its heat by contact, is assumed to have originally derived its cold, in like manner from expansion. This is evident, for although he recognizes the effect of radiation into space, he assumes that this loss is compensated by counter radiation. The upper regions are, he says, exposed to the

* The italics are mine.

radiation of the sun on the one side, and of the earth's lower atmosphere on the other, and there is no proof that these do not equal the surface temperature. And again, when the air descends in high latitudes to the earth's surface an amount of heat will be evolved by compression equal to that which it lost when it rose from the equator.

Professor Newcomb has misapprehended not only my meaning, but also the chief reason why the air in the upper regions is so intensely cold. Any one who has read what I have stated in pp. 35–40, 'Climate and Time,' regarding the temperature of space will readily understand what I mean by the temperature of the upper regions. By the temperature of stellar space, it is not meant that space itself is a something possessed of a given temperature, say −239° F. It simply means the temperature to which a body would fall were it exposed to no other source of heat than that of radiation from the stars. By the temperature of the upper regions, I mean the temperature to which air in those regions sinks in consequence of loss from radiation into space. It is mainly to this cause, and not to the loss from expansion, as Professor Newcomb assumes, that the intense cold of the upper air is due. The air in that region has got beyond the screen which protected it when at the earth's surface, and it then throws off its heat into space during twelve hours of night, getting no return from without except from the radiation of the stars. And even at noon-day, as I have endeavored to show in Appendix, p. 551, the rays of a burning sun over head would not be sufficient to raise the temperature of the air up to the freezing point. But the recent observations of Professor Langley prove that the loss of heat from radiation is in reality far greater than I had anticipated. He says: "The original observations, which will be given at length, lead to the conclusion that in the absence of an atmosphere the earth's temperature of insolation would at any rate fall below −50° F., by which it is meant that, for instance, mercury would remain a solid under the vertical rays of a tropical sun were radiation into space wholly unchecked, or even if, the atmosphere existing, it let radiations of all wave-lengths pass out as easily as they come in."—'*Nature*,' Aug. 3d, 1882.

The temperature of the upper atmosphere, even after making allowance for heat received from below, must in this case be at least nearly 80 degrees below the freezing point. The quantity of heat lost by expansion must, therefore, be trifling compared with that lost by radiation; and although the heat lost by expansion is fully restored by compression, yet the air would reach the earth deprived almost entirely of the heat with

which it left the equator. All that it could possibly give back would simply be the heat of compression, and this would hardly be sufficient to raise air at −50° F. to the freezing point. How then can the polar regions be greatly the better of air from the equatorial regions? Professor Newcomb says: "If the upper current be as great as is commonly supposed, it must be as powerful as ocean currents in tending to equalize the temperature of the globe." How can this be?

Why the mean temperature of the Ocean should be greater than that of the Land.—"Another proposition," he says, "which the author attempts to prove, reasoning which seems equally inconclusive, is that the mean temperature of the ocean is greater than that of the land over the entire globe." I certainly never attempted to prove that the mean temperature of the ocean is *greater* than that of the land over the entire globe. The very chapter to which he here refers, and which he is about to criticise, was written to explain why the mean temperature of the southern or water hemisphere is less than that of the northern or land hemisphere. What I attempted to prove was, not that the mean temperature of the ocean *is* greater than that of the land, but that were it not for certain causes the mean temperature of the ocean *ought* to be greater than that of the land in equatorial regions as well as in temperate and arctic regions. In other words, the object of the chapter is to prove that the mean temperature of the southern or water hemisphere is less than that of the northern or land hemisphere, not, as is generally supposed, because the former is mainly water and the latter land, but because of the enormous amount of heat transferred from the former to the latter hemisphere by means of ocean currents; and that were it not for this transference the temperature of the water would exceed that of the land hemisphere. And it is in order to prove this that the "four *à priori* reasons" which Professor Newcomb criticises were adduced. The first of these is as follows—

First.—'The ground stores up heat only by the slow process of conduction, whereas water, by the mobility of its particles and its transparency for heat-rays, especially those from the sun, becomes heated to a considerable depth rapidly. The quantity of heat stored up in the ground is thus comparatively small, while the quantity stored up in the ocean is great.'*

These sentences are considered unworthy of criticism. Are they really so unworthy? Let us examine them a little more closely. It is in consequence of the sun's rays being able to

* 'Climate and Time,' p. 90.

penetrate to a great depth that the amount of heat stored up by the ocean is so great, and it is to this store that its warmth during winter is mainly due. The water is diathermanous for the rays of the sun, but it is not so, for reasons well known, for the rays of water itself. The upper layers of the ocean will allow a larger portion of the radiation from the sun to pass freely downward, but they will not allow radiation from the layers underneath to pass freely upwards. These upper layers, like the glass of a green-house, act as a trap to the sun's rays, and thus allow the water of the ocean to stand at a higher temperature than it would otherwise do. Again, the slowness with which the ocean thus parts with its heat enables it to maintain that comparatively high temperature during the long winter months. And again, it is to the mobility of the particles of water, the depth to which the heat penetrates, and the rapidity with which it is absorbed, that those great currents of warm water become possible. Were the waters of the ocean like the land not mobile, and were only a few inches at the surface reached by heat from the sun, there could be no Gulf Stream, or any great transference of heat from the Southern to the Northern hemisphere, or from equatorial to temperate and polar regions, by means of oceanic circulation.

Second.—'The air is probably heated more rapidly by contact with the ground than with the ocean; but, on the other hand, it is heated far more rapidly by radiation from the ocean than from the land. The aqueous vapor of the air is to a great extent diathermanous to radiation from the ground, while it absorbs the rays from water and thus becomes heated.'

To this Professor Newcomb objects as follows: "If then the air is really heated by contact with the ground more rapidly than by contact with the ocean, it can only be because the ground is hotter than the ocean, which is directly contrary to the theory Mr. Croll is maintaining." What I maintained was that were it not for certain causes the *mean annual* temperature of the ocean would be higher than that of the land. During the day and also during the summer the surface of the ground is hotter than that of the ocean; and the air, of course, will be heated more rapidly by *contact* with the former than with the latter. But this does not prove that the air is not more rapidly heated by *radiation* from the ocean than from the land. Professor Newcomb says: "The statement that the aqueous vapor of the air is diathermanous to radiation from land, but not to that from water, is quite new to us, and very surprising." I am surprised that he is not acquainted with the fact, and also with its physical explanation. This will

help to account for his inability to perceive how radiation from the ocean may heat the air more rapidly than radiation from the land, even though the surface of the latter may be at a higher temperature than that of the former.

He says: "The rapidity with which the heating process goes on depends on the difference of temperature, no matter whether the heat passes by conduction or by radiation." This statement will hardly harmonize with recent researches into radiant heat. It is found that the rapidity with which a body is heated by radiation depends upon the absorbing power of the body; and the absorbing power again depends upon the quality of the heat rays. Professor Tyndall, for example, found that in the case of vapors, as a rule, absorption *diminishes* as the temperature rises. With a platinum spiral heated till it was barely visible, the absorption of the vapor of bisulphide of carbon was 6·5, but when the spiral was raised to a white heat, the absorption was reduced to 2·9. A similar result took place in the case of chloroform, formic ether, acetic ether, and other vapors. The physical cause of this is well known.

If the aqueous vapor of the air, he says, be more diathermanous to radiation from land than from water, as I have stated, then I assigned directly contrary effects to the same cause. For, "reasoning as in (1) he, Mr. Croll, would have said that the air over the land, owing to its transparency for the heat rays from the land, becomes heated to a greater height rapidly, while the air over the ocean not being transparent, can acquire heat from the ocean only by the slow process of convection." I would have said no such thing. Radiation from the surface of the land will, no doubt, penetrate more freely through the aqueous vapor than radiation from the ocean, but the aqueous vapor will not absorb the radiation of the land so rapidly as that of the ocean, for the ocean gives off that quality of rays which aqueous vapor absorbs most rapidly.

This is not in opposition to what I have stated in reason (1), for if the ground were transparent to the sun's rays like water, evidently the total quantity of heat absorbed by it would be greater than that by the ocean. But radiation from the sun heats only the surface of the ground, all below the surface depends for its supply on the slow process of conduction, whereas the ocean is heated by direct radiation to great depths. Consequently the total quantity of heat absorbed by the ocean, say per square mile, in a given time, is greater than that by the land.

Third.—'The air radiates back a considerable portion of its heat, and the ocean absorbs this radiation from the air more readily than the ground does. The ocean will not reflect the heat from the aqueous vapor of the air, but absorbs it, while the ground does the opposite. Radiation from the air, therefore, tends more readily to heat the ocean than it does the land.'

"Here we have," he says, "the air giving back to the ocean the same heat which it absorbs from it, and thus heating it." If Professor Newcomb means by this same heat the same amount of heat, then I believe in no such thing. But if his meaning be that here we have the air giving back to the ocean a quantity of the heat which it absorbed from it, then he is certainly correct in supposing that this is affirmed by me. But this is a conclusion which no physicist could for a moment doubt. To deny this would be to contradict Prevost's well-known theory of exchanges. Did the air throw back to the ocean none of the heat which it derives from it the entire waters of the ocean would soon become solid ice. In fact, as we have seen, mercury would not remain fluid and every living thing on the face of the globe would perish.

He states that reason fourth seems to be little more than a repetition of reason second in a different form. It is, however, much more than that. It is a demonstration that were it not for the causes to which I have alluded the mean temperature of the water hemisphere ought to be higher than that of the land hemisphere, and for this reason I shall here give the section in full.

Fourth.—'The aqueous vapor of the air acts as a screen to prevent the loss by radiation from water, while it allows radiation from the ground to pass more freely into space; the atmosphere over the ocean consequently throws back a greater amount of heat than is thrown back by the atmosphere over the land. The sea in this case has a much greater difficulty than the land has in getting quit of the heat received from the sun; in other words, the land tends to lose its heat more rapidly than the sea. The consequence of all these circumstances is that the ocean must stand at a higher mean temperature than the land. A state of equilibrium is never gained until the rate at which a body is receiving heat is equal to the rate at which it is losing it; but as equal surfaces of sea and land receive from the sun the same amount of heat, it therefore follows that in order that the sea may get quit of its heat as rapidly as the land, it *must stand at a higher temperature* than the land. The temperature of the sea must continue to rise till the amount of heat thrown off into space equals that received from the sun; when this point is reached equilibrium is established and the temperature remains stationary. But, owing to the greater difficulty that the sea has

in getting rid of its heat, the mean temperature of equilibrium of the ocean must be higher than that of the land; consequently the mean temperature of the ocean, and also of the air immediately over it, in tropical regions should be higher than the mean temperature of the land and the air over it.'

Since the publication of 'Climate and Time,' the accuracy of this conclusion has been confirmed in a remarkable manner from recent researches on the actual mean temperature of the two hemispheres, the details of which have been given by Mr. Ferrel in his 'Meteorological Researches,' Washington, 1877. It is found that the mean temperature of the northern or land hemisphere is higher than that of the southern or water hemisphere up only to about latitude 35°, and that beyond this latitude the mean temperature of the water hemisphere is the greater of the two. At latitude 40° the mean temperature of the southern hemisphere is 1°·4 higher than that of the same parallel on the northern hemisphere. At latitude 50° the difference amounts to 4°·4, while at latitude 60° the mean temperature of the southern hemisphere is actually 6° higher than that of the northern on the same parallel. The mean temperatures of the two hemispheres are as follows:

Lat.,	0°	10°	20°	30°	40°	50°	60°	70°	80°
Northern,	80°·1	81°·0	77°·6	67°·6	56°·5	43°·4	29°·3	14°·4	4°·5
Southern,	80°·1	78°·7	74°·7	66°·7	57°·9	47°·8	35°·3	----	----

From the above table we see that it is only in that area lying between the equator and latitude 35° that the southern hemisphere has a lower mean temperature than the northern. But it is from this area that the enormous amount of heat transferred to the northern hemisphere is mainly derived. Were the transference of heat to cease the temperature of this area would be very considerably raised, and that of the corresponding area on the northern hemisphere lowered. The result would doubtless be that the southern hemisphere, down to the equator, would then be warmer than the northern. But even as things are, as Mr. Ferrel remarks, "the mean temperature of the southern hemisphere is the greater of the two," the mean temperature of the southern being 60°·89, and that of the northern 59°·54.

Heat cut off by the Atmosphere.—Professor Newcomb says further: "Another idea of the author which calls for explanation is that solar heat absorbed by the atmosphere is entirely lost, so far as warming any region of the globe is concerned." This is no idea of mine. My idea is not that the heat cut off is entirely lost but merely that the *greater part* is lost. A large portion of the heat is reflected, and of that absorbed one-

half, perhaps, is radiated back into space and lost, in so far as the earth is concerned.

Tables of Eccentricity.—Referring to my tables of eccentricity of the earth's orbit he says: "That there are from time to time such periods of great eccentricity is a well-established result of the mutual gravitation of the planets, but whether the particular epochs of great and small eccentricity computed by Mr. Croll are reliable, is a different question." I may here mention that Professor McFarland, of the Ohio State University, Columbus, a few years ago, undertook the task of re-computing every one of the 150 periods given in my tables, and he states that, except in one instance, he did not find an error to the amount of ·001.*

"The data for this computation," continues Professor Newcomb, "are the formulæ of Le Verrier, worked out about 1845,† without any correction either for the later corrections to the masses of the planets or for the terms of the third order, subsequently discussed by Le Verrier himself. The probable magnitude of these corrections is such that reliance cannot be placed upon the values of eccentricity computed without reference to them for epochs distant by merely a million of years."

In regard to this objection I may mention that the whole subject of the secular variations of the elements of the planetary orbits has been re-investigated by Mr. Stockwell, taking into account the disturbing influence of the planet Neptune, the existence of which was not known at the time Le Verrier's investigations were made. Professor McFarland, with the aid of Mr. Stockwell's formulæ, has computed all the periods in the tables referred to above, and on comparing the results found by both formulæ, he states that "the two curves exhibit a general conformity throughout their whole extent." And his computations, I may state, extend from 3,260,000 years before 1850 to 1,260,000 years after that date; or, in other words, over a period of no fewer than 4,520,000 years,‡ thus showing that Professor Newcomb's objection falls to the ground.

Influence of Winter in Aphelion.—I have maintained that at a time when the eccentricity is high and the winter occurs in aphelion, the great increase in the sun's distance and in the length of the winter would have the effect of causing a large

* American Journal of Science, vol. xi, p. 456 (1876).

† Le Verrier's formulæ were worked out several years before 1845.

‡ In this laborious undertaking Professor McFarland computed, by means of both formulæ, the eccentricity of the earth's orbit and the longitude of the perihelion for no fewer than 485 separate epochs. See American Journal of Science, vol. xx, p. 105 (1880).

increase in the quantity of snow falling during that season. This very obvious result follows as a necessary consequence from the fact that the moisture which now falls in the form of rain would then fall as snow. But Professor Newcomb actually states that he cannot accept the conclusion that this would lead to more snow.

Influence of a Snow-covered Surface.—I have argued that this accumulation of snow would lower the summer temperature, and tend to prevent the disappearance of the snow, and have assigned three reasons for this conclusion :—

First.—Direct radiation. The snow, for physical reasons well known, will cool the air more rapidly than the sun's rays will heat it. This is shown from the fact that in Greenland, a snow and ice-covered country, a thermometer exposed to the direct radiation of the sun has been observed to stand above 100°, while the air surrounding the instrument was actually 12° below the freezing point. Professor Newcomb and also Mr. Hill * regard the idea that this could in any way favor the accumulation of snow as absurd. They think that in fact it would have directly the opposite effect. They have perceived only one half of the result. It is quite true, as they affirm, that the cooling of the air by the snow will not prevent the melting of the snow, but the reverse. There is, however, another and far more important result overlooked in their objection. If the snow and ice-covered surface keeps the temperature of the air, in summer, below the freezing point, which it evidently does in Greenland and in the Antarctic continent, the moisture of the air will fall as snow and not as rain. No doubt this is the chief reason why in those regions, even in the middle of summer, rain seldom falls, the precipitation being almost always in the form of snow, although at that very season the direct heat of the sun is often as great as in India. Were the snow and icy mantle removed, a snow shower in summer would be as rare a phenomenon in those regions as it would be in the south of England.

Second.—'The rays which fall on snow and ice are to a great extent reflected back into space. But those that are not reflected, but absorbed, do not raise the temperature, for they disappear in the mechanical work of melting the ice.'

This reason is also regarded as absurd. The heat of the sun during the perihelion summer would, he says, suffice to melt the whole accumulation of winter snow in three or four days. "The reader," he continues, "can easily make a computation of the incredible reflecting power of the snow and of the unex-

* Geological Magazine for January, 1880, p. 12.

ampled transparency of the air required to keep the snow unmelted for three or four months." Incredible as it may appear to Professor Newcomb, I shall shortly show that a less amount of snow than the equivalent of the two feet of ice which he assumes does actually, in some places, defy the melting power of a tropical sun. But he misapprehends my reasoning here also, by overlooking the more important factor in the affair, namely, the keeping of the air in the summer below the freezing point. The direct effect that this has in preventing the sun from melting the snow and ice will be discussed shortly, but the point to which I wish at present to direct special attention is the fact that if the air is kept below, or even at the freezing point, snow will fall and not rain. Snow is a good reflector of heat, consequently a large portion of the sun's rays falling on the snow and icy surface is reflected back to space. The aqueous vapor of the air, on the other hand, as the vibrations of its molecules agree in *period* with those of the snow and ice, cuts off a large portion of the heat *radiated* by the snow surface; but here in the case of reflection under consideration the rays are not cut off; for the *reflected* rays are of the same character as the incident rays which pass so freely through the aqueous vapor. And in respect to the remaining rays which are not reflected but absorbed by the snow, they do not manage to raise the temperature of the snow above the freezing point. Consequently the air is kept in the condition most favorable for the production of snow.

Third.—'Snow and ice lower the temperature by chilling the air and condensing the vapor into thick fogs. The great strength of the sun's rays during summer, due to his nearness at that season, would, in the first place, tend to produce an increased amount of evaporation. But the presence of snow-clad mountains and an icy sea would chill the atmosphere and condense the vapor into thick fogs. The thick fogs and cloudy sky would effectually prevent the sun's rays from reaching the earth, and the snow in consequence, would remain unmelted during the entire summer.'

On this Professor Newcomb's criticism is as follows: "Here he, Mr. Croll, says nothing about the latent heat set free by the condensation, nor does he say where the heat goes to which the air must lose in order to be chilled. The task of arguing with a disputant who in one breath maintains that the transparency of the air is such that the rays reflected from the snow pass freely into space, and in the next breath that thick fogs effectually prevent the rays ever reaching the snow at all, is not free from embarrassment."

If he really supposes my meaning to be that the air is so transparent as to allow the incident and reflected rays of the

sun to pass freely without interruption, while at the same time and in the same place the air is not transparent but filled with dense fogs which effectually cut off the sun's rays and prevent them from reaching the earth, then I do not wonder that he should feel embarrassed in arguing with me. But if he supposes my meaning to be, as it of course is, that those two opposite conditions, existing at totally different times, or in totally different places at the same time, should lead to similar results, namely, the cooling of the air and consequent conservation of snow, then there is no ground whatever for any embarrassment about the matter.

"We might therefore show," he states, "that if the snow, air, fog, or whatever throws back the rays of the sun into space is so excellent a reflector of heat, it is a correspondingly poor radiator, and the same fog which will not be dissipated by the summer heat will not be affected by the winter's cold, and will therefore serve as a screen to prevent the radiation of heat from the earth during the winter."

There are few points in connection with terrestrial physics which appear to be so much misunderstood as that of the influence of fogs on climate. One chief cause of these misapprehensions is the somewhat complex nature of the subject arising from the fact that aqueous vapor acts so very differently under different conditions. When the vapor exists in the air as an invisible gas, we have often an intensely clear and transparent sky, allowing the sun's rays to pass to the ground with little or no interruption; and if the surface of the ground be covered with snow, a large portion of the incident rays are reflected back into space without heating either the snow or the air. The general effect of this loss of heat is, of course, to lower the general temperature. But when this vapor condenses into thick fogs it acts in a totally different manner. The transparency to a great extent disappears, and the fog then cuts off the sun's rays and prevents them from reaching the ground. This it does in two different ways. 1st. Its watery particles, like the crystals of the snow, are good reflectors, and the upper surface of the mass of fog on which the rays fall acts as a reflector, throwing back a large portion of the rays into stellar space. The rest of the rays which are not reflected enter the fog and the larger portion of them are absorbed by it. But it will be observed that by far the greater part of the absorption, if not nearly all of it, will take place in the upper half of the mass. This is a necessary result of a recognized principle in radiant heat known as the "sifting" of the rays. The deeper the rays penetrate into the fog the less will be the amount of heat absorbed. If the depth of the mass be great,

absorption will probably entirely disappear before the surface of the ground is reached. The fog will begin, of course, to radiate off the heat thus absorbed, but as it is the upper half of the mass which has received the principal part of the heat, the most of this heat will be radiated upward into stellar space, and like the reflected heat entirely lost in so far as heating the earth is concerned. A portion will also be radiated downward, some of which may reach the ground, but the greater portion will be re-absorbed in its passage through the mass. We have no means of estimating the amount of heat which would thus be thrown off into space by reflection and radiation; but it is certainly great. I think we may safely conclude that in places like South Georgia and Sandwich Land where fogs prevail to such an extent during summer, one half at least of the heat from the sun never reaches the ground. A deprivation of sun heat of a much less extent than this would certainly lower the summer temperature of these places far below the freezing point, were it not for a compensating cause to which I shall now refer, viz: the heat "trapped" by the fog. The fog, although it prevents a large portion of the sun's heat from ever reaching a place, at the same time prevents to a great extent that place from losing the little heat which it does receive. In other words, it acts as a screen preventing the loss of heat by radiation into space. But the heat thus "trapped" never fully compensates for that not received, and a lowering of temperature is always the result.

Had all these considerations been taken into account by Professor Newcomb, Mr. Hill, Mr. Searles Wood and others, they would have seen that I had by no means over-estimated the powerful influence of fogs in lowering the summer temperature.

The influence of fogs on the summer temperature is a fact so well established by observation that it seems strange that any one should be found arguing against it.

Heat evolved by Freezing.—There is one objection to which I may here refer and which has been urged by nearly all my critics. It is said, correctly enough, that as water in freezing evolves just as much heat as is required to melt it, there is on the whole no actual loss of heat; that whatever heat may be absorbed in the mechanical work of melting the snow just as much was evolved in the formation of the snow. Consequently it is inferred, in so far as climate is concerned, the one effect completely counterbalances the other. This inference, sound as it may at first sight appear, has been so well proved to be incorrect by Mr. Wallace that I cannot do better than quote his words:

"In the act of freezing, no doubt water gives up some of its heat to the surrounding air, but *that air still remains below the freezing point* or freezing would not take place. The heat liberated by freezing is, therefore, what may be termed low-grade heat—heat incapable of melting snow or ice; while the heat absorbed while ice or snow is melting is high-grade heat, such as is capable of melting snow and supporting vegetable growth. Moreover, the low-grade heat liberated in the formation of snow is usually liberated high up in the atmosphere, where it may be carried off by winds to more southern latitudes, while the heat absorbed in melting the surface of snow and ice is absorbed close to the earth and is thus prevented from warming the lower atmosphere, which is in contact with vegetation. The two phenomena, therefore, by no means counterbalance or counteract each other, as it is so constantly and superficially asserted that they do."—*Island Life*, p. 140.

The Fundamental misconception.—I come now to a misapprehension which more than any other has tended to prevent a proper understanding of the causes which lead to the conservation by snow. Whatever the eccentricity of the earth's orbit may be the heat received from the sun during summer is more than sufficient to melt the snow of winter. Consequently, it is assumed, no permanent accumulation of snow can take place. This objection, as expressed by Mr. Hill, is as follows: "We have no reason to suppose that at present in the northern hemisphere more snow or ice is anywhere formed in winter than is melted in summer. With greater eccentricity less heat than now would be received in winter, but exactly as much more in summer. More snow would therefore be formed in the one half of the year, but exactly as much more be melted in the other half. The colder winter and the warmer summer would exactly neutralize each other's effects, and on the average of years no accumulation could begin. *Prima facie*, therefore, high eccentricity will not account for glacial periods."* In the language of Professor Newcomb it is as follows: "During this perihelion summer the amount of heat received from the sun by every part of the northern hemisphere would suffice to melt from four to six inches of ice per day over its entire surface; that is, it would suffice to melt the whole probable accumulation in three or four days. The reader can easily make a computation of the incredible reflecting power of the snow and of the unexampled transparency of the air required to keep the snow unmelted for three or four months."

* Geological Magazine, January, 1880, p. 12.

It is assumed, in this objection, that because the heat received from the sun by an area is more than sufficient to melt all the snow that falls on it, no permanent accumulation of snow and ice can take place. It is assumed that the quantity of snow and ice melted must be proportional to the heat received. Suppose that on a certain area a given amount of snow falls annually. The amount of heat received from the sun per annum is computed, and after the usual deduction for that cut off by the atmosphere has been made, if it be found that the quantity remaining is far more than sufficient to melt the snow, it is then assumed that the snow must be melted, and that no accumulation of snow and ice year by year in this area is possible. To one approaching this perplexing subject for the first time such an assumption looks very plausible, but a little reflection will show that it is most superficial. The assumption is at the very outset totally opposed to known facts. Take the lofty peaks of the Himalayas and Andes as an example. Few, I suppose, would admit that at these great elevations as much as fifty per cent of the sun's heat could be cut off. But if fifty per cent reaches the snow this would be sufficient to melt fifty feet of ice, and this no doubt is more than ten times the quantity which actually requires to be melted. Notwithstanding all this the snow is never melted but remains permanent. Take as another example South Georgia in the latitude of England. Suppose we assume that one-half of the sun's heat is cut off by the clouds and fogs which prevail to such an extent in that place, still the remaining half would be sufficient to melt upwards of thirty feet of ice, which is certainly more than the equivalent of all the snow which falls; yet this island is covered with snow and ice down almost to the seashore during the whole year. Take still another example, that of Greenland. The quantity of heat received between latitudes 60° and 80°, which is that of Greenland, is, according to Meech, one-half that received at the equator; and were none cut off, it would be sufficient to melt fifty feet of ice. The annual precipitation on Greenland in the form of snow and rain, according to Dr. Rink, amounts to only twelve inches, and two inches of this he considers is never melted but is carried away in the form of icebergs. Mr. Hill maintains* that owing to the great thickness of the air traversed by the sun's rays and the loss resulting from the great obliquity of reflection, the amount of heat reaching the ground would be insufficient to melt more than sixteen feet of ice. Supposing we admit this estimate to be correct still this is nineteen times more than is actually melted. The sun melts

* Geological Magazine, April, 1880.

only ten inches notwithstanding the fact that it has the power to melt sixteen feet.

In short, there is not a place on the face of the globe where the amount of heat received from the sun is not far more than sufficient to melt all the snow which falls upon it. If it were true, as the objection assumes, that the amount of snow melted is proportional to the amount of heat received by the snow then there could be no such thing as perpetual snow.

The reason why the amount of snow and ice melted is not necessarily proportional to the amount of heat received is not far to seek. Before snow or ice will melt its temperature must be raised to the melting point. No amount of heat, however great, will induce melting to begin unless the intensity of the heat be sufficient to raise the temperature to the melting point. Keep the temperature of the snow below that point and though the sun may shine upon it for countless ages it will still remain unmelted. It is easy to understand how the snow on the lofty summits of the Himalayas and the Andes never melts. According to the observations made at Mount Whitney, to which reference has already been made, the heat of even a vertical sun would not be sufficient at these altitudes to raise the temperature of the snow to near the melting point; and thus melting, under these conditions, is impossible. The snow will evaporate but it cannot melt. But owing to the frozen condition of the snow even evaporation will take place with extreme difficulty. If the sun could manage to soften the snow crystals and bring them into a semi-fluid condition, evaporation would, no doubt, go on rapidly; but this the rays of the sun are unable to do. Consequently, we have only the evaporation of a solid which, of course, is necessarily small.

It may here be observed that at low elevations, where the snow fall is probably greater, and the amount of heat received even less than at the summits, the snow melts and disappears. Here, again, the influence of that potent agent, aqueous vapor, comes into play. At high elevations the air is dry and allows the heat radiated from the snow to pass into space; but at low elevations a very considerable amount of the heat radiated from the snow is absorbed by the aqueous vapor which it encounters in passing through the atmosphere. A considerable portion of the heat thus absorbed by the vapor is radiated back on the snow; but the heat thus radiated being of the same quality as that which the snow itself radiates is on this account absorbed by the snow. Little or none of it is reflected like that received from the sun. The consequence is that the heat thus absorbed accumulates in the snow till melting takes place. Were the amount of aqueous vapor possessed by the

atmosphere sufficiently diminished, perpetual snow would cover our globe down to the sea shore. It is true that the air is warmer at the lower than at the higher levels, and by contact with the snow must tend to melt it more at the former than at the latter position. But we must remember that the air is warmer mainly in consequence of the influence of aqueous vapor, and that were the quantity of vapor reduced to the amount in question, the difference of temperature at the two positions would not be great.

But it may be urged as a further objection to the foregoing conclusion that, as a matter of fact, on great mountain chains the snow line reaches to a lower level on the side where the air is moist than on the opposite side where it is dry and arid: as, for example, on the southern side of the Himalayas, and on the eastern side of the Andes, where the snow line descends 2,000 or 3,000 feet below that of the opposite or dry side.

But this is owing to the fact that it is on the moist side that by far the greatest amount of snow is precipitated. The moist winds of the southwest monsoon deposit their snow almost wholly on the southern side of the Himalayas, and the southeast trades on the east side of the Andes. Were the conditions in every respect the same on both sides of these mountain ranges, with the exception only that the air on one side was perfectly dry, allowing radiation from the snow to pass without interruption into stellar space, while on the other side the air was moist and full of aqueous vapor absorbing the heat radiated from the snow, the snow line would in this case undoubtedly descend to a lower level on the dry than on the moist side. Melting would certainly take place at a greater elevation on the moist than on the dry side, and this is what would mainly determine the position of the snow line.

The annual precipitation on Greenland, as we have seen, is very small; scarcely one-half that of the dryest parts of Great Britain. This region is covered with snow and ice, not because the quantity of snow falling on it is great, but because the quantity melted is small; and the reason why the snow does not melt is not that the amount of heat received during the year is unequal to the work of melting the ice, but that, mainly through the dryness of the air, the snow is prevented from rising to the melting point. The very cause which prevents a heavy snowfall protects the little which does fall from disappearing. The same remarks apply to the antarctic regions.

In South Georgia and Fuegia, where clouds and dense fogs prevail during nearly the whole year, the permanent snow and

ice are due to a different cause. Here the snowfall is great and the amount of heat cut off enormous; but this alone would not account for the non-disappearance of the snow and ice. For, notwithstanding this, the heat received is certainly more than sufficient to melt all the snow which falls, great as that amount may be. The real cause is that the heat received is not sufficiently intense to raise the temperature to the melting point. More heat is actually received by the snow than is required to melt it, but it is dissipated and lost before it can manage to raise the temperature of the snow to the melting point; consequently the snow is not melted. Here snow falls in the very middle of summer, but snow would not fall unless the temperature were near the freezing point.

Foregoing principles applied to the case of the Glacial epoch.—Let us now apply the foregoing principles to the case of the Glacial epoch. As winter then occurred in aphelion during a high state of eccentricity, that season would be much longer and colder than at present. Snow in temperate regions would then fall in place of rain, and although the snowfall during the winter might not be great, yet as the temperature would be far below the freezing point, what fell would not melt. As heat, which produces *evaporation*, is just as essential to the accumulation of snow and ice as is cold, which produces *condensation*, after the sun had passed the vernal equinox and summer was approaching the consequent rise of temperature would be accompanied by an increase in the snowfall. A melting of the snow would also begin, but it would be a very considerable time before the amount melted would equal the daily amount of snow falling. Rain, alternating with snow showers, would probably result; and, for some time before mid-summer, snow would cease and give place entirely to rain. Melting would then go on rapidly, and by the end of the summer the snow would all disappear except on high mountain summits such as those of Scotland, Wales and Scandinavia. Before the end of autumn, however, it would again begin to fall. Next year would bring a repetition of the same process, with this difference, however, that the snow line would descend to a lower level than on the previous year. Year by year the snow line would continue to descend till all the high grounds became covered with permanent snow.

It would not require a very great amount of change from the present condition of things to bring about such a result. A simple lowering of the temperature, which would secure that snow instead of rain, should fall for six or eight months in the year would suffice; and this would follow as a neces-

sary result from an increase of eccentricity. Now if all our mountain summits were covered with permanent snow down to a considerable distance, the valleys would soon become filled with local glaciers. In such a case we should then have more than one-half of Scotland, a large part of the north of England and Wales, with nearly the whole of Norway covered with snow and ice. Here a new and powerful agent would come into operation which would greatly hasten on a glacial condition of things. This large snow and ice-covered surface would tend to condense the vapor into snow. It would during summer chill the air and produce dense and continued fogs, cutting off the sun's rays and leading to a state of things approaching to that of South Georgia, which would much retard the melting of the snow.

It is a great mistake, as I have repeatedly shown, to suppose that the perihelion summers of the Glacial epoch could be hot. No snow and ice-covered continent can enjoy a hot summer. This is clearly shown by the present condition of Greenland. Were it not for the ice the summers of North Greenland, owing to the continuance of the sun above the horizon, would be as warm as those of England; but, instead of this, the Greenland summers are colder than our winters, and snow during that season falls more or less nine days out of ten. But were the ice covering removed a snow shower during summer would be as great a rarity as it would be with us. On the other hand, cover India with an ice sheet, and the summers of that place would be colder than those of England.

When the high grounds of Scotland and Scandinavia, with those of the northern parts of America, became covered with snow and ice, and the eccentricity went on increasing, a diminution of the Gulf Stream and a host of other physical agencies, all tending toward a glacial condition of things, would be brought into operation. This would ultimately and inevitably lead to a general state of glaciation without the aid of any of those additional geographical changes of land and water which some have supposed. This will be shown more fully when we come to examine Mr. Alfred R. Wallace's theory.

The Mutual Reaction of the Physical Agents.—Those who think that the agencies to which I refer would not by themselves bring about a glacial condition appear to overlook a most important and remarkable circumstance regarding their mode of operation, to which I have frequently alluded in 'Climate and Time' (pp. 74–77) and other places. The circumstance is this. The physical agencies in question not only all lead to one result, viz: an accumulation of snow and ice, but their efficiency in bringing about this result is actually

strengthened by their mutual reaction on one another. In physics the effect reacts on the cause. In electricity and magnetism, for example, cause and effect in almost every case mutually act and react upon each other, but the reaction of the effect tends to weaken the cause. Those physical agents to which I have referred, no doubt, in their mutual actions and reactions obey the same law; but, in reference to *one particular result*, viz: the accumulation and conservation of snow, those mutual reactions strengthen one another. This is not reasoning in a circle, as Mr. Searles Wood supposes; for the reaction of an effect may on the whole weaken the cause, and yet in regard to a particular result it may strengthen it. In the case under consideration the agents not only act in one direction, but their efficiency in acting in that one direction is strengthened by their mutual reactions. This curious circumstance throws a flood of light on the causes which tended to bring about the Glacial epoch.

To begin with we have a high state of eccentricity. This leads to long and cold winters. The cold leads to snow, and although heat is given out in the formation of the snow, yet the final result is that the snow intensifies the cold: it cools the air and leads to still more snow. The cold and snow bring a third agent into play—*fogs*—which act still in the same direction. The fogs intercept the sun's rays, this interception of the rays diminishes the melting power of the sun, and so increases the accumulation. As the snow and ice continue to accumulate, more and more of the rays are cut off; and on the other hand, as the rays continue to be cut off, the *rate* of accumulation increases, because the quantity of snow and ice melted becomes thus annually less and less. In addition, the loss of the rays cut off by the fogs lowers the temperature of the air and leads to more snow being formed, while again the snow thus formed chills the air still more and increases the fogs. Again, during the winters of a Glacial epoch, the earth would be radiating its heat into space. Had this loss of heat simply lowered the temperature, the lowering of the temperature would have tended to diminish the rate of loss; but the result is the formation of snow rather than the lowering of the temperature.

Further, as snow and ice accumulate on the one hemisphere they diminish on the other. This increases the strength of the trade winds on the cold hemisphere and weakens those on the warm. The effect of this is to impel the warm water of the tropics more to the warm hemisphere than to the cold. Suppose the northern hemisphere to be the cold one, then as the snow and ice begin gradually to accumulate, the ocean currents

of that hemisphere, more particularly the Gulf Stream, begin to decrease in volume, while those on the southern or warm hemisphere begin *pari passu* to increase.* This withdrawal of heat from the northern hemisphere favors the accumulation of snow and ice, and as the snow and ice accumulate the ocean currents decrease. On the other hand, as the ocean currents diminish, the snow and ice still more accumulate. Thus the two effects in so far as the accumulation of snow and ice is concerned mutually strengthen each other.

The same process of mutual action and reaction takes place among the agencies in operation on the warm hemisphere, only the result produced is diametrically opposite to that produced in the cold hemisphere. On this warm hemisphere action and reaction tend to raise the mean temperature and diminish the quantity of snow and ice existing in temperate and polar regions.

The primary cause of all these physical agencies being set in operation is a high state of eccentricity of the earth's orbit, and with a continuance of that state a glacial epoch becomes inevitable.

The Explanation begins with Winter.—Mr. Hill asks why I always begin in my explanation with the aphelion winter rather than with the perihelion summer. The reason is that the character of the summer is determined by that of the winter and not the winter by that of the summer. It is true that to a certain extent the influence is mutual, but the effect of the summer on the winter is trifling in comparison with that of the winter on the summer. To begin our explanation with the summer would be like beginning at the end of a story and telling it backward.

Herr Woeikof on the cause of Glaciation.—In an article by A. Woeikof on 'Glaciers and Glacial Periods in their relations to Climate' (Nature, March 2d, 1882), it is maintained that the chief cause which leads to the formation of snow, and consequently to a glacial condition, is a low surface-temperature of the sea surrounding or adjoining the land. When the surface temperature of the water much exceeds the freezing-

* Professor Dana has shown that in North America those areas which at present have the greatest rain-fall are, as a rule, the areas which were most glaciated during the Glacial epoch. Mr. Searles V. Wood maintains that this fact is inconsistent with the theory that the Glacial period was due to the cause to which I attribute it. I am totally unable to comprehend how he arrives at this conclusion. Supposing the Gulf Stream, as I have maintained, were greatly diminished during the Glacial period, still I think it would follow, other things being equal, that the areas which now have the greatest rain-fall would during that period probably have the greatest snow-fall, and consequently the greatest accumulation of ice. The amount of precipitation might be less than at present, but this would not prevent the areas which had the greatest snow-fall from being most covered with ice.

point the vapor, he says, evaporated from the sea and condensed on the land will be rain and not snow, but when the temperature of the water is near the freezing-point, snow will be the result. A diminution, for example, in the heat brought by the Gulf Stream that would very greatly lower the surface temperature of the sea surrounding Great Britain would, he says, bring about a heavy snow-fall and lead to permanent snow and ice. Again he maintains "as there is no reason to suppose that the surface-temperature of the sea would be lower during winter in aphelion and high eccentricity, it follows that there will not be more snow than now in countries where rain is the rule, even in winter, all other things equal."

There is surely a fallacy lurking under this theory of M. Woeikof. Snow instead of rain is not, as he supposes, owing to the low temperature of the water from which the vapor is derived, but to the low temperature of the air where the vapor is precipitated. Of course, when the surface of the sea is near the freezing-point, the air over the sea and the adjoining land is usually also not far from the freezing-point, and consequently the precipitation is more likely to be snow than rain. If the air be cold as it generally is over a snow and ice covered country, a high temperature of the adjoining seas, were this possible, would greatly increase the snow-fall because it would greatly augment the quantity of vapor which would be available for snow.

Edinburgh, Scotland, May 1st, 1883.

ART. XXX.—*Communications from the U. S. Geological Survey, Rocky Mountain division.* IV. *On minerals of the Cryolite group recently found in Colorado;* by WHITMAN CROSS and W. F. HILLEBRAND.

IN this Journal, for October, 1882,* we announced, in connection with the description of zircon and other minerals from the neighborhood of Pike's Peak, that cryolite and several allied fluorides had been identified from the same region and would form the subject of a paper at an early day. Shortly after the above date there appeared in the "Zeitschrift für Krystallographie," etc., an interesting and valuable paper by Professor P. Groth† containing the results of a searching investigation into the crystallographical and chemical properties, not only of cryolite and its alteration products, but also of the few

* Third series, vol. xxiv, p. 281.

† P. Groth, "Beiträge zur Kenntniss der natürlichen Fluorverbindungen," Zeitschrift für Krystallographie, vii, pp. 375–388 and 457–493.

allied minerals occurring independently. The chemical analyses of the above article, which had been previously published apart, were made by J. Brandl* upon material selected and crystallographically examined by Professor Groth. By a critical review of the existing literature, and by renewed investigations in doubtful cases, it was hoped to clear away the uncertainty which had hung about some of the members of the group, and as the available material was, for the most part, far better than had been examined before, it was possible to obtain very satisfactory results.

In the present paper, frequent reference will necessarily be made to the results of Messrs. Groth and Brandl. Owing to better material, we are in some cases able to give new or supplementary data, and only in one instance, namely, in regard to the composition of pachnolite, is there any discrepancy between our results and those contained in the articles above cited.

Locality and occurrence.—The point at which these minerals occur may be described as at the northeast base of St. Peter's Dome, a small conical peak, due west of Cheyenne Mountain, and near the Pike's Peak toll-road, which is now completed from Colorado Springs to the Seven Lakes, at the base of Pike's Peak. Although several miles in a straight line from the mountain proper, the locality may be considered as within the "Pike's Peak region."

The country rock of this whole district is a coarse, reddish, biotite granite, of a general type which is common in the Colorado Range of the Rocky Mountains, and is, in all probability, a part of the Archæan formation. A specimen collected near St. Peter's Dome shows largely predominant reddish feldspar, brilliant, black biotite, and quartz, the latter rather subordinate, in small grains between the other minerals. The feldspar consists largely of an intergrowth of orthoclase and albite, somewhat after the manner of perthite. The rock disintegrates very readily (l. c., p. 281), and the study of the geognostic relations in the region is much hindered by the gravelly debris which covers many slopes almost entirely.

The cryolite locality lies on the southeast border of the extensive district within which Amazon stone and its associated minerals are so abundantly found in cavities in the granite. In the immediate vicinity astrophyllite and associated zircon occur in granitic veins, and also, as does the arfvedsonite, in veins of white quartz.

The minerals to be described occur in two veins of massive white quartz, and in each case they were discovered by prospectors. The two veins are scarcely more than one-third of a

* J. Brandl, Sitzungsbericht der königl. bayr. Akademie der Wissenschaften zu München, 1882, p. 118, and Annalen der Chemie, ccxiii, p. 1.

mile apart, and their continuation is so concealed by soil and debris that it cannot be seen what their relation to each other and to the other veins may be. They differ, however, so greatly in the minerals they contain and in the manner of occurrence of the latter, as to make it improbable that they are united.

In the vein which we will designate vein A, cryolite, pachnolite, thomsenolite, gearksutite, prosopite and probably ralstonite appear, with but rare associated minerals. In vein B, on the other hand, prosopite, fluorite and mixed fluorides occur intimately associated with zircon, kaolinite and a greenish yellow mica. As the minerals of the two veins are so distinct, we can best consider them in the groups afforded by the veins themselves.

Vein A.

At this place a very steep incline was sunk on an outcrop of white quartz. At about ten feet from the surface a small, solid mass of fluorides, some two feet in thickness, was struck, and below it came massive quartz again. The boundaries of this mass are quite irregular, and its lateral extent is still unknown.

This small body of mineral, which was no doubt wholly cryolite originally, is now composed of the same alteration products which are known in connection with the Greenland cryolite, some of them in better development than is known in this latter locality. The only associated minerals observed to penetrate the cryolite mass are astrophyllite and columbite, the former being almost wholly altered to a dull green substance which is not homogeneous and whose composition is unknown. The astrophyllite springs from the granitic wall of the vein in the form of blades several inches long, and zircon was noticed with it in the mass at the base of the blades, while never seen imbedded directly in the cryolite. The columbite is in small, rhombic prisms of the type found with the Amazon stone, and occurs quite rarely in isolated crystals. The cryolite decomposition products adjacent to the astrophyllite will be described later (page 289).

Adjoining the quartz, the cryolite is always decomposed, and is generally replaced by a massive mixture of pachnolite and thomsenolite, but not infrequently the alteration has gone still farther and a white powder of extreme fineness, like purest kaolin, fills the space between quartz and more solid pachnolite. This is the gearksutite which, when wet, as it usually is here, makes a thick paste or mud. The description of the minerals will begin with cryolite and proceed through the different stages of alteration here exhibited.

Cryolite.

Within the mass of fluorides in vein A there is a surprisingly large amount of fresh cryolite. It can be obtained in solid pieces several inches in diameter, and the process of alteration can be readily followed from the perfectly fresh material. It occurs in massive aggregates of crystalline individuals which, as shown by the continuous cleavage surfaces, are often two to three inches in diameter, and are never very small. The freshest substance has usually a delicate pink or even decidedly rose color; less frequently a faint greenish tinge, and none so far obtained has the snowy whiteness or the clearness of the Greenland cryolite. The color disappears on heating, leaving the cryolite pure white. Cleavage, in three directions, very nearly at right angles to each other, is quite distinct, but although as plainly marked as in the Greenland mineral, actual separation parallel to the cleavage planes is not so easily effected as is normal. The cause of this lies, undoubtedly, in the complicated polysynthetic twin structure revealed by the microscope.

No crystals of cryolite have been found, and a thoroughly satisfactory study of the laws of twinning which appear in this massive material would require much more time than we have been able to devote to it. In some of the thin sections which have been prepared the relations are simple and admit of perfectly satisfactory explanation. Thus, in some sections parallel to the plane of most perfect cleavage, a lamellar polysynthetic twin structure like that of common plagioclase is visible. These lamellæ lie parallel to one of the two visible systems of cleavage lines, are continuous, approximately of equal width, appear most distinctly when parallel to the principal section of one of the crossed Nicols, and extinction takes place simultaneously, so far as can be determined by the weak polarization of the mineral, at an angle of about 45° from the twinning line and parallel to the diagonals of the prism as indicated by the cleavage fissures. This structure evidently indicates the law of twinning frequently noticed in the Greenland cryolite, namely, that parallel to *I* (110). The same twinning law is seen in many sections parallel to one of the less perfect or prismatic cleavage surfaces. A similar laminated structure appears, and extinction takes place at 30° to 33° from the twinning line, in opposed directions in alternate laminæ, 31° 15′ being the angle of extinction with the edge of *I*, theoretically required in sections parallel to a prism face (Groth). Such a section usually shows two systems of laminæ situated nearly at right angles to each other but seldom, if ever, crossing. The second system seems probably to represent a twinning parallel to the base. Associated with these two systems

is frequently a third crossing the other two at nearly 45°, as seen in a section parallel to *I*. This may be caused by twinning parallel to a hemiorthodome, but, as stated above, the laws here represented have not as yet been fully investigated.

Chemical investigation.—For all analyses of this and the following minerals the greatest care was taken to have the purest of reagents. The fluorine was in all cases determined by the Wöhler-Fresenius method, with the slight modifications introduced by Brandl,* except that the iron plate, instead of the oil bath, was used for heating. The sulphuric acid was obtained of the highest degree of concentration and purity by distillation from a platinum retort. The water was determined by absorption in a chloride of calcium tube, the mineral having been heated in a tube with either oxide of lead or carbonate of sodium, the results being the same whether one or the other was used.

The cryolite, of which the analysis is here given, possessed the specific gravity 2·972 at 24° C., was faintly pink in color, and contained as a visible impurity the oxide of iron represented in the analysis:

Fe_2O_3	0·40
Al	12·90
Ca	0·28
Na	32·40
H_2O	0·30
F	53·55†
	99·83

The presence of a slight amount of water indicates incipient alteration. Purer material was subsequently obtained but a second analysis seemed unnecessary.

Alteration of cryolite.—The alteration of the cryolite proceeds in two ways, producing the same minerals in the end. By one process the principal cleavage fissures are utilized by the solutions which effect the change, and thin walls are formed of a white crystalline substance. The next step seems to be the bodily removal of the cryolite matter between these walls, leaving a network of partitions in the three directions of the chief cleavages of the original cryolite. These partitions or walls are lined by minute crystals. The second mode of alteration proceeds from the neighboring quartz, and from the boundaries of the different crystalline individuals of the cryolite, the result being a compact crystalline mass of a faint bluish tinge. The material at hand illustrates the two processes and their products about equally well, and they are often naturally combined.

* Annalen der Chemie, ccxiii, p. 1.

† As the mean of 53·35, 53·46, 53·55 and 53·85.

Pachnolite.

a. From the thin walls.—The microscopical examination of the walls and membranes produced by the first mode of alteration of the cryolite shows them to be coated by many minute but perfect, colorless and transparent, prismatic crystals which are usually placed at right angles to the central plane of the wall, though sometimes in irregular manner. They reach a maximum length of 2^{mm} by a thickness of $0{\cdot}2$ to $0{\cdot}4^{mm}$. The crystals are occasionally yellow in color, while retaining their transparency, the color being doubtless owing to some matter produced by the alteration of the astrophyllite which penetrates all such specimens. The crystallographical identification of these crystals with pachnolite is quite certain, for upon placing them in vertical position under the microscope the prism angles can readily be measured and correspond closely to 81° 24′ and 98° 36′, the theoretical angles of pachnolite (see Groth, l. c. p. 463). The prism $I(110)$ and base $O(001)$ are in all cases the chief faces, accompanied frequently by a hemiorthodome, and very rarely by a clinodome, both very slightly developed. The former is considered to be $-\bar{1}(101)$ from data given below. Pyramid faces have not been seen upon crystals of this growth. Although all detached crystals show when examined in polarized light as lying upon a prism face, an oblique twinning plane in the prismatic zone,* still a projecting angle upon the base can but rarely be seen. The reflecting surfaces of the thinnest walls are composed of innumerable small facets of rhombic outline—the basal planes of the very low prisms. In fragments from some of the thinnest walls, placed horizontally under the microscope and observed in polarized light, twinning parallel to the shorter (ortho-) diagonal could easily be seen. The crystals are usually quite equally bisected by the twinning line. The central portion of these walls is rather dull white and probably represents the alteration product on cleavage planes of the original cryolite, while the crystals themselves were formed during or after the removal of the intermediate cryolite substance.

Thomsenolite is but rarely present with pachnolite on these walls. The few crystals found correspond to the pachnolite in size and possess a prism angle of nearly 90°, and a very perfect cleavage parallel to the base.

Material from a network of thin walls covered by pachnolite crystals was subjected to chemical analysis, yielding the result under II. p. 281.

b. From the bluish alteration product.—The pale bluish mass formed by the second mode of decomposition has in great part a regular crystalline structure produced by a more or less per-

* Groth, l. c. p. 464.

fect intergrowth of pachnolite individuals in three directions approximately at right angles to each other. Nearly simultaneous reflection over the greater part of certain irregular surfaces makes this relation plain. By the examination of such a surface with a loupe, one can usually identify a number of rhombic facets which are nearly or quite coincident in position with striated planes, the two corresponding to *O* and *I* of different individuals. Such a structure is also illustrated by the crystals in the numerous small cavities occurring in the massive material.

These cavities are of wholly irregular shape and reach a maximum observed diameter of 3 to 4^{cm}. The crystals lining them are often very perfect and are occasionally 2 or 3^{mm} in length, with a thickness of 1^{mm} or less. The study of these crystals proves that, as in the preceding case, most of them must be referred to pachnolite, although thomsenolite is sparingly present. These pachnolite crystals differ in habit from those already described in that the pyramid is usually prominent, being, however, in nearly every case truncated by the basal plane; in fact, crystals without *O* are very rare. The rhombic section of the prism is everywhere plain. Although every prism on being optically tested showed a twinning plane in the prismatic zone, the low projecting angle of 179° 20′ upon *O* could seldom be distinctly seen. Many crystals are somewhat extended parallel to one pair of prism faces. A hemiorthodome of the same order as the pyramid is sometimes developed and probably corresponds to that noticed upon the crystals of the foregoing type. Most of the crystals of these cavities, while very perfect and distinctly recognizable as pachnolite, are too small and their surfaces are too frequently covered by minute crystals of a later growth to be available for measurements with the goniometer.

Whether thomsenolite is mixed with pachnolite in the massive portion or not is difficult to determine. It is certainly in comparatively small quantity if present, and in the cavities all the recognizable crystals are deposited upon the pachnolite and are apparently distinctly later in formation.

Two specimens of the bluish, massive material when carefully examined proved to contain pachnolite in a form allowing of more exact crystallographic, optical and chemical investigation.

The first of these, which we will designate specimen A, is about 8×5×2^{cm} in size, is somewhat more coarsely granular than the variety described, and possesses in an eminent degree the regular structure there observed. While quite compact in the greater part of the specimen there are portions in which the grains are more loosely aggregated together and parts of

various individuals have perfectly developed crystal faces. In some minute cavities a few crystals with quite perfect terminations were found, and upon these some faces were sufficiently large and polished to admit of measurements with a Fuess reflection goniometer. These crystals are about 1mm long and nearly the same in thickness. They show I (110), O (001), with subordinate -1 (111) and a negative pyramid determined as $-3\cdot\bar{3}$ (311). They are all twinned parallel to $i\text{-}\bar{i}$ (100) and the low projecting angle upon O is sometimes distinctly visible.

The angles given in the following table are all means of numerous closely agreeing measurements, and demonstrate the crystallographical identity of the mineral under discussion with pachnolite:

Angle.	Crystal *a*.	Crystal *b*.	Other Crystals.	Calculated.
$I \wedge I$ (110 ∧ 1$\bar{1}$0)	81° 19′	81° 22′	81° 18′	81° 24′
$I \wedge O$ (110 ∧ 001)		90° 21′	90° 21′	90° 20′
$-1 \wedge O$ (111 ∧ 001		116° 39′	116° 30′	116° 30′
$-3\cdot\bar{3} \wedge -3\cdot\bar{3}$ (311 ∧ 3$\bar{1}$1	138° 45′		138° 45′	138° 52′ 14″
$-3\cdot\bar{3} \wedge I$ (311 ∧ 110)	149° 03′			149° 7′ 19″
$O \wedge O$ (001 ∧ 001) twin		179° 21′		179° 20′

The face $-3\cdot\bar{3}$ was observed on a number of distinct twin crystals with projecting angle on O, and in several cases on both of the negative angles.

Upon one side of specimen A are a few thomsenolite crystals, distinguishable by their prism angle of nearly 90°. They lie irregularly and seem to be later in formation than the pachnolite. Upon them are deposited minute prosopite crystals and indistinct alteration products. In the mass of this specimen no thomsenolite can be detected, while all individuals with partially free development are plainly pachnolite. Analysis III of table (page 281), was made on material obtained from the clear, loosely granular portions of specimen A, and although it was necessary to include many transparent grains of irregular shape in order to obtain a desirable amount of substance, there is no doubt in our own minds but that thomsenolite was wholly absent from the material analyzed.

The second specimen (B), from which especially good material was obtained, had a seam 2cm thick of coarse granular structure running through it, and upon splitting it open along this seam two surfaces of water-clear, loosely adhering crystalline grains of pachnolite were exposed, with the regular arrangement described. Actual development of crystal faces other than the prism is rarer than in specimen A, but the size of the grains, reaching 5mm in length by 1–3mm in thickness, is such as to admit of the preparation of thin sections for optical examination, and also gave absolutely pure material for chemical

analysis. None of the crystals upon which the faces were well formed were superior to those from specimen A, and only measurements of the prism angles were made. The face $-3\cdot\bar{3}$ was not observed at all.

The optical properties of these pachnolite crystals are such as to leave no room for doubt concerning their crystallographical identity with the mineral described by Professor Groth as pachnolite. Several sections were prepared as nearly parallel to the clinopinacoid as possible. These exhibit in all cases a twin structure, and this is frequently polysynthetic. The twinning lines are straight and lie parallel to the vertical axis. Extinction takes place at 21° 30′—22° or 68°—68° 30′ from the twinning line, in opposed directions in alternate laminæ. According to Professor Groth the bisectrix is in the plane of symmetry and inclined 68° 5′ forward from the vertical axis. Sections parallel to the base show the twinning structure also, the line lying parallel to the orthodiagonal.

A large part of the purest crystals and pyramids from this specimen were used for chemical analysis and repeated water determinations, the results of which are given below (IV, page 281).

Chemical investigation. — Previous to the analysis by J. Brandl[1] of pachnolite carefully selected by Professor Groth, pachnolite and thomsenolite were considered to possess the same chemical composition. The results of all earlier analyses, excluding such as referred to manifestly very impure material, while frequently deviating materially from the figures required by theory for the formula NaF, CaF_2, AlF_3, H_2O, still agree on the whole very well, as shown in the accompanying table, and fully justified the belief in the chemical identity of the two species:

	Thomsenolite.		Pachnolite.					
	Wöhler.[2]	König.[3]	Knop.[4]	König.[3]	vom Rath.[5]		Hagemann.[6]	Calculated for NaF, CaF_2, AlF_3, H_2O.
Al	13·43	13·74	13·14	12·50	13·46	12·93	10·37	12·32
Ca	17·84	16·79	17·25	18·17	18·10	17·99	17·44	17·98
Na	10·75	10·10	12·16[7]	10·23	10·68	12·06	12.04	10·34
H_2O	8·20	9·00	9·60	8·19			8·63	8·10
F	49·78	50·37	50·79	51·54			51·15	51·26
	100·00	100·00	102·94	100·63			99.63	100·00

[1] Ann. d. Chem., ccxiii, p. 6.
[2] Neues Jahrbuch für Min., etc., 1876, p. 851.
[3] Proceedings Acad. Sci. of Philad., 1876, p. 42.
[4] Annalen der Chemie und Pharmacie, cxxvii, p. 61.
[5] Sitzungsbericht d. niederrhein Ges. für Natur- und Heilkunde, 1860, xx, p. 141.
[6] This Journal, 1866, II, xli, p. 119. [7] Also 10·80 and 10·81.

Wöhler's analysis was entirely confirmed some years subsequently by Jannasch* in Gottingen, who subjected to analysis pure thomsenolite selected by Professor Klein.

In view of the above, the analysis of pachnolite by Brandl, showing results agreeing well with those required by the formula NaF, CaF_2, AlF_3 was calculated to cause no little surprise. Professor Groth accepts without question the anhydrous nature of pachnolite and endeavors to explain away the opposing evidence shown in the foregoing table by assuming that the supposed homogeneous material analyzed was contaminated largely with thomsenolite. As in no published analysis does the percentage of water fall below 7 per cent, this assumption necessitates a most improbable admixture of thomsenolite. In those cases where the percentage of water equals or exceeds that required for thomsenolite, the presence of gearksutite is suggested by Professor Groth as a possible explanation. Even on this supposition the percentage of foreign admixture could not fall below 50 per cent, in which extreme case the whole of the impurity must be gearksutite, an amount which it is difficult to conceive should have escaped the notice of such observers as König, vom Rath and Knop, the latter of whom expressly says his analysis was made upon material identified as pachnolite.† Notwithstanding the difficulty of explaining the agreement between the previous analyses of pachnolite and thomsenolite on the assumption of the anhydrous nature of the former, the correctness of Brandl's analysis was not at first questioned by us.

In the course of the present investigations, the compact bluish material (see p. 276) having the specific gravity 2·980 at 22½° C. was first analyzed, the crystallized pachnolite not having yet been observed. The results of analysis as given under I below, agreed in the main so well with the figures required for the formula NaF, CaF_2, AlF_3, H_2O, that no hesitation was felt in considering the mineral to be thomsenolite, probably slightly contaminated with fluorite. Later, the crystalline coating on the thin walls produced by the first mode of alteration of the cryolite, the crystals forming which had not yet been identified crystallographically as pachnolite, were subjected to analysis with the results given under II. Here again the identity with thomsenolite seemed clear. It was not until the analysis of perfectly transparent fresh crystals, and crystal fragments, all taken from specimen A above described gave the results shown under III, that the possibility of the first analyses having been also made upon pachnolite, was suggested. That this was, however, so in the case of No. II sub-

* Neues Jahrbuch für Min., etc., 1877, p. 808.
† Neues Yahrbuch für Min., etc., 1876, p. 850.

sequent careful examination fully revealed, though the crystals analyzed were not entirely free from foreign admixture. It seems certain also that the compact bluish portions (I) consist almost entirely of pachnolite, although it cannot be positively asserted that some thomsenolite may not be intergrown with it. That no possible doubt might exist in the mind of any one as to the homogeneity of the material used for analysis III, a further analysis was made upon crystals from specimen B above described, particular care being taken to identify each as pachnolite by the rhombic section. The results of this analysis appear under IV.

	Hillebrand.							Brandl.†	Calculated for NaF, CaF_2, AlF_3, H_2O.
	I.		II.		III.		IV.		
Al	12·02		13·02	13·01	12·23		12·36	13·60	12·32
Ca	19·32		15·27	15·17	18·06		18·04	18·83	17·98
Mg	0·13		1·53						
Na	10·43		10·28		10·23		10·25	11·73	10·34
Ka			0·13						
H_2O	7.87	7·95	8·64	8·79	8·10	8·11	8·05		8·10
F					51·33	51·28	51·30*	55·69	51·26
					99·95		100·00	99·85	100·00

Further determinations of water on material from specimen B gave 7·95; 7·99; 8·14, and 8·15 per cent. Still other determinations, some on material every fragment of which showed the rhombic section, others on material taken at random from the crystalline mass gave results between 7·90 and 8·20 per cent. The spec. grav., at 17° C. of the perfectly pure material, as the mean of four determinations varying between 2·963 and 2·968, was 2·965. A single determination on another portion equally pure at 22° C. gave 2·962. The transparent crystals, as well as all the other portions analyzed, decrepitated violently on heating in a test tube, the walls became lined with the white deposit so characteristic of thomsenolite and pachnolite, and much water was given off. Hence, it appears that the pachnolite from Pike's Peak and thomsenolite are identical in chemical composition, unless the fact of all analyses of thomsenolite showing slightly more water than required for the formula NaF, CaF_2, AlF_3, H_2O may indicate, as suggested by Professor Groth, a partial replacement of fluorine by hydroxyl in that mineral. Should this prove to be the case, a plausible explanation of the difference in crystallization of the two minerals is offered without recourse to the theory of dimorphism.

A satisfactory explanation of Brandl's results so opposed to those presented by all earlier analyses and the ones above

* By difference. † Ann. d. Chem. ccxiii, p. 6.

given is impossible, but it may be well to call attention to the fact that Brandl was obliged to make his determinations of fluorine and the metals upon quantities of 0·1106 gr. and 0·1430 gr. weight respectively, whereas, material was not wanting for the present analyses, the determinations having been made upon weights of from 0·3 gr. to 0·75 gr. It nowhere appears that a direct test for water was made upon the material furnished by Professor Groth. The latter, it is true, remarks (l. c. p. 461), "Ausserdem bildet sich bei letzterem (Thomsenolith) in den kälteren Theilen des Rohrs ein Wasserbeschlag, welcher beim Erhitzen reinen Pachnolithes natürlich ausbleibt." The absence of water does not, however, seem to be hereby proven, but simply assumed from the close approximation to 100 of Brandl's results exclusive of water. Brandl himself says water is wanting, but does not mention if this was ascertained by direct experiment. The small amount of material at his disposal renders it not improbable that no direct test was made.

Since the conclusion of the above investigations we have, through the kindness of Mr. Albert F. Damon, President of the Pennsylvania Salt Manufacturing Company, obtained specimens of Greenland cryolite and its alteration products. From one of these specimens was removed a large number of small, needle-like, pyramidally-terminated, twinned crystals with a rhombic prismatic section, showing in fact, precisely the common occurrence* and ordinary habit† of pachnolite as described by Professor Groth. These crystals, slightly yellowish in color but quite transparent, were individually examined under the microscope, all such as did not show beyond a doubt the above described habit being excluded. They were then tested in a small glass tube for water. Decrepitation ensued on heating and the walls of the tube became lined with a white powder, and also with a deposit of water in such quantity as to preclude the possibility of its having been derived from but a small portion of the material experimented upon.

Other forms of pachnolite.—In some very cellular specimens whose walls run irregularly and seemingly without reference to the cleavage of the original cryolite, are crystals of pachnolite of different habit.

One or two of these cavities show crystals corresponding in size to those upon the thin walls, but exhibiting, each and every one, a reëntering angle on the free termination. In such little crystals the basal planes are prominent, and they are bounded on the inside by a pyramid and dome, doubtless -1 (111) and -1–$\bar{1}$ (101). Outward there sometimes appears another pyramid (1?) though the prismatic faces themselves usually form a sharp edge with *O*. All these crystals are too small for measure-

* P. Groth, l. c., p. 461. † Ibid, p. 462.

ment, but as the appearance described is such as would be normal for the termination by which the twin crystals are commonly attached, it seems admissible to consider the faces as O, -1, -1–$\bar{\imath}$ and probably 1. On many twins of this kind, the outer or positive angles between I and O are replaced by two faces greatly resembling those of $-3\cdot\bar{3}$ (311) and although entirely too minute for measurement, it is probable that the form $3\cdot\bar{3}$ ($\bar{3}$11) is here represented. The crystals of Greenland pachnolite are always attached by the end with the reëntering angle according to Groth.

Secondary formations in cavities.—In many cavities arising from both modes of decomposition of the cryolite, a second series of minerals has been deposited, chiefly as a whitish, easily crumbling aggregate of minute crystalline grains, which are recognizable under the microscope as thomsenolite, pachnolite and a mineral of the isometric system. The little thomsenolite crystals are frequently doubly terminated, and show the monoclinic symmetry plainly through the more prominent development of the negative pyramid. The pachnolite crystals are short and stout, showing in most cases only I(110), O(001) and -1–$\bar{\imath}$ (101). A number of large pachnolite crystals were observed which, by low powers of the microscope, seemed coated with a crystalline dust, whose particles are resolved by a power of 700–800 diameters into most perfect cubic crystals, modified by the octahedron. This is also the form of the regular crystals in the granular mass above mentioned, and were it not for a fact to be considered shortly, we should not hesitate to advocate the probable identity of these crystals with ralstonite, described by Brush * as occurring in a very similar manner upon thomsenolite from Greenland. The largest of the crystals occurring as described, is less than 1^{mm} in diameter, and no pure material even for qualitative tests can be obtained.

A new fluoride.—Just at the close of our investigations, a mineral was found, occurring sparingly in a few specimens, which seems to be very different from any known species. It was found in small cavities in the massive pachnolite, as a compact irregular mass, colorless, but not perfectly clear and exhibiting but seldom traces of crystalline form. In one specimen, however, the mass of the mineral was covered by small, rounded, crystal-like projections, which seemed like crystals of the regular system. An examination with a loupe showed the absence of recognizable faces, but such particles when broken off and tested under the microscope, proved to be fully isotropic. A few faces found on one crystal seem to belong to

* G. J. Brush, this Journal, III, ii, p. 30, 1871; also P. Groth, l. c., p. 471.

cube and octahedron, and particles detached from the same are isotropic in action in polarized light. Supposing that this substance must be ralstonite in exceptional development, enough material for the following partial chemical analysis was selected, being carefully freed from attached particles of pachnolite and other anisotropic minerals, by microscopical examination. The Al, Ca and Mg were accurately determined, the Ka and Na, owing to an unfortunate mishap, only approximately. No water could be detected by direct test upon a small portion. Fluorine was present in quantity, and the percentage given below is calculated on the assumption that the metals are fully combined with it.

Al	11·40
Ca	0·72
Mg	0·22
Ka	28·94
Na	9·90
F	46·98
	98·16

From the above data may be derived a formula analogous to that of cryolite, in which about two-thirds of the sodium is replaced by potassium. Further investigation will be made upon this most interesting mineral as soon as better material can be obtained. The presence of this regular mineral, naturally renders it impossible to refer the tiny crystals observed upon pachnolite to ralstonite with any degree of certainty, but we are inclined to believe that the two minerals are not identical.

Gearksutite.

This mineral, first observed by Hagemann* on compact thomsenolite, and described as earthy and kaolin-like in aspect, dull, white, opaque and of hardness 2, seems to be so rare in connection with the Greenland fluorides, that no one has had material for further examination. Groth† found it in very small quantity among the minerals at his disposal, but could not obtain enough for analysis. He, however, found that it consisted of very minute microscopic needles, with oblique extinction, and considers it as undoubtedly a definite species.

Among the minerals from St. Peter's Dome, gearksutite is quite abundant. It is not formed from other minerals by molecular replacement, but is deposited from solution, in the cavities upon fresh crystals of pachnolite, etc. Smaller cavities are sometimes filled by it, and on the contact with the quartz it is specially developed.

In appearance, it corresponds closely to the description of the Greenland mineral, as given by Dana, the resemblance to the

* Dana, System of Mineralogy, 5th Ed., p. 130. † Groth, l. c., pp. 460 and 481.

purest, finest kaolin being especially remarkable. Examined microscopically, gearksutite is seen to consist, as stated by Prof. Groth, of exceedingly minute colorless needles, the average length of which is less than 0·02^{mm}, and the thickness less than 0·002^{mm}, and apparently possessing oblique extinction.

Chemical investigation.—Gearksutite was found by Hagemann (l. c.), to possess the following composition:

Al	15·52
Ca	19·25
Na	2·46
H_2O	20·22
F	41·18
	98·63

An examination of the above results shows that the atomic ratio of Al : Ca+Na_2 : Fl is 1 : 1 : 4, instead of 1 : 1 : 5 which would represent complete saturation and require about 12 per cent more fluorine than was found. On the assumption that the missing fluorine is replaced in the mineral by oxygen or hydroxyl, there should appear a much greater loss than the analysis indicates. An error is therefore evident, probably in connection with the determination of the fluorine, or water, or both, in consequence of which the construction of a satisfactory formula has heretofore been impossible.

The material for the following analyses was first partially crushed, then freed from admixed heavier particles of foreign matter by triturating in a beaker with water, the impurities falling to the bottom of the vessel, while the light gearksutite remained suspended in the liquid and was removed by decantation. By repeating this operation a great many times, a product was finally obtained entirely free from all foreign admixture. It was allowed to settle completely, the supernatant liquid poured off and the residue dried first on the water bath, then at 100° C. Thorough pulverization of this residue is a difficult matter, as it flattens out under the pestle, forming flakes which strongly resist the pulverizing action. This is of little moment, however, since the flakes are so spongy as to offer no hindrance to attack by sulphuric acid. Two analyses were made from the same sample, with the results tabulated below. In *b*, sodium and potassium were not determined.

	a.	*b.*	Mean.
Al	15·33	15·30	15·31
Ca	22·29	22·32	22·30
Na	0·10	----	0·10
Ka	0·04	----	0·04
H_2O	15·54	15·39	15·46
F	42·14	42·01	42·07
	95·44	----	95·28
Loss as oxygen	4·56	----	4·72
	100·00		100·00

Taking the figures in the third column and combining the fluorine with the calcium, sodium, potassium, and, as far as possible with the aluminium, there remains of the latter 5·32 per cent, requiring 4·66 per cent of the oxygen, an amount agreeing very closely with that obtained above by difference and making the sum total almost exactly 100.

22·30	Ca requires	21·18 F.
0·10	Na "	0·08
0·04	Ka "	0·02
9·99	Al "	20·79
9·98	Al_2O_3 "	42·07
15·46	H_2O	
42·07	F	
99·94		

The agreement of the above analysis with that of Hagemann, after substituting in the latter for the sodium its equivalent in calcium, is very close, with the single exception of the water. As his analysis was manifestly erroneous in some particular, the assumption of the identity of gearksutite with the mineral here discussed is fully justified, supported as it is by the similarity in occurrence, appearance and physical characteristics. His error would then consist in having obtained from four to five per cent too much water, a result not difficult of explanation, in the case of a hydrated fluoride, if no precaution was taken to prevent the escape of fluorine.

Substituting in the mean of analyses *a* and *b* for sodium and potassium their equivalent of calcium, and dividing the percentages by the atomic weights, the atomic ratio is found to be as given below :

Al	15·31	÷ 27·4	=	0·559
Ca	22·41	÷ 40	=	0·560
H_2O	15·46	÷ 18	=	0·859
O	4·66	÷ 16	=	0·291
F	42·07	÷ 19	=	2·214

The ratio of Al : Ca : F is here nearly as 1 : 1 : 4, the same as found by Hagemann. Subtracting from the atomic value for water, an amount 0·291 equal to that for oxygen, in order to form with the latter hydroxyl, the result is as given under I, while under II appears the ratio referred to calcium as unity.

	I.	II.	
Al	0·559	1·00	
Ca	0·560	1·00	
H_2O	0·568	1·01	
HO	0·582	1·04	4·99
F	2·214	3·95	

It will be seen that by combining hydroxyl and fluorine the ratio Al : Ca : H_2O : (F, OH) is 1 : 1 : 1 : 5 and the formula for the mineral becomes CaF_2, $Al(F, OH)_3$, H_2O, in which the fluorine and hydroxyl combined with the aluminium stand nearly in the proportion 2 : 1. Were the latter proportion exactly fulfilled, the formula might be written $3CaF_2$, $2AlF_3$, $Al(OH)_3$, $3H_2O$ requiring the percentages:

Al 15·36, Ca 22·42, F 42·60, O 4·49, H_2O 15·13=100·00

Of the 15·46 per cent of water found by analysis, 5·24 per cent has been considered in the foregoing as basic. While this amount may, on theoretical grounds alone, enter into the inner constitution of the mineral as basic water, the remainder cannot, but must be water of crystallization. That a portion of the water is basic, is rendered more than probable by the fact that at 300° C. some is still retained. In this connection, the following experiments were made. 0·5677 gr. of the mineral, not however from the same sample as that used for analysis, dried first at 100° C. and contained in a platinum crucible, was exposed in an air bath during 145 hours to temperatures ranging from 100° C. to 300° C., the weight being taken at intervals averaging ten hours each. The results in brief showed that at 145° C., the loss was but 0·35 per cent, at 230° C. only 0·92 per cent, at 250° C. 7·02 per cent, after prolonged heating at 265–270° C. 9·49 per cent, and at 295° C. 13·92 per cent. As no further loss occurred after six hours' heating at 295–300° C., a portion of the residue, which still retained its original appearance, was subjected to a quantitative test for water, of which 1·76 per cent was found. This added to the 13·92 per cent driven off below 300° C. made the total 15·68 per cent. Since this is slightly higher than the mean of the previous results, it seemed possible that some fluorine might have escaped. The remainder of the residue was therefore tested quantitatively for fluorine, of which was found 40·60 per cent, thus proving the correctness of the surmise. A similar experiment with the same general results was made upon a smaller portion of another sample. A comparison of the full results of both experiments showed that by sufficiently prolonged heating at approximately 270° C., all the water of crystallization could be driven off, also that by still further heating at little, if any, higher temperature, the basic water began to escape, but was not entirely expelled, even after many hours' exposure to a temperature of 295–300° C.

In the Journal of the Chemical Society for 1883, page 140, Walter Flight describes a mineral obtained from the cryolite bed of Greenland. "It is made up of a congeries of minute white transparent crystals, mostly broken up and lying entan-

gled among each other in every sort of direction, which gives the mass an appearance of opacity much resembling that of kaolin or chalk."

Chemical analysis showed it to consist of

					Equivalents.
Al	16·23	with F	33·64	= 49·87	0·59
Ca	22·39	" "	21·27	= 43·66	1·12
Na	0·43	" "	0·33	= 0·76	
				94·29	
			Water	5·71	0·63
				100·00	

The fluorine seems to have been calculated for the metals, and the water was found by difference.

From the above data the author obtains the formula $2CaF_2$, Al_2F_6, $2H_2O$,* and considering the mineral new, names it *evigtokite*. Mr. Flight seems to have overlooked the description of gearksutite in Dana's System of Mineralogy, and Prof. Groth's remarks upon the same (l. c. pp. 481 and 493), else the very fair agreement of his analytical data for the metals with those of Hagemann, and the similarity in occurrence, appearance and physical characteristics of the two minerals must have led to at least a suspicion of their identity. There can hardly exist a doubt that Flight has analyzed gearksutite, and that the name evigtokite is therefore to be dropped.

Prosopite.

This rare species hitherto unobserved in association with the cryolite minerals, and known only in connection with the tin-bearing veins of Altenberg, in Saxony, has been identified in both veins at St. Peter's Dome. It is most abundant in vein B, and the chief description of it will come under that head, but close examination has proved its presence also among the minerals of vein A.

Both of the coarsely crystalline specimens of pachnolite, above described as A and B, have prosopite upon them. Specimen B is, in parts, in process of alteration to a dull, white, porous substance, with little cavities in which are minute crystals of prosopite. These are colorless, transparent, tabular in shape, showing $i\text{-}\bar{i}$ (010) predominant, I (110), 1 ($\bar{1}11$) and $-2\text{-}\bar{2}$ (211), agreeing exactly in form and optical behavior with those determined as prosopite in vein B. In two other specimens of pachnolite, prosopite tablets may be seen upon certain granular surfaces when decomposition of the pachnolite has already begun. The crystals are usually attached by the prismatic edges, although free and perfect terminations are to be found. Gearksutite was noticed upon them in one specimen, and the

* Probably a printer's error. It should read $2CaF_2$, Al_2F_6, H_2O.

position of prosopite in the series of hydrous fluorides is doubtless between thomsenolite and gearksutite. No material for chemical tests could be procured from the specimens described.

The penetration of the fluoride mass by astrophyllite blades springing from the side of the vein has been mentioned (p. 273). Here the cryolite has been altered to cellular pachnolite, according to the first mode described, and this has for the greater part given way to other products or been dissolved and carried away, leaving the astrophyllite blades more or less free or imbedded in gearksutite and other soft, crumbling material. The free blades usually have a coating composed of a little purplish fluorite immediately adjacent to the blades, and over this a nearly colorless or slightly yellowish substance. At and toward the base of the astrophyllite blades the latter is present in roundish aggregates made up, as shown by the loupe, of clear tablets, in more or less radiate arrangement. The crystal form is here quite obscure, but the general appearance was so suggestive of prosopite that by sacrificing the best specimen enough material was obtained for determination of the bases and of fluorine, with the result given below—V, p. 293.

Vein B.

Description.—The minerals found in this vein have been exposed by the "Eureka" tunnel which has been driven in upon it for one hundred feet or more. Unfortunately the walls are so blackened by the smoke from the blasting that the relations of the different mineral bodies encountered to each other cannot be clearly seen, and the data here given are derived from the study of the material upon the dump and from a partial examination of the tunnel walls with the aid of a hammer.

The main part of the vein is white, massive quartz with here and there immense individuals of pinkish feldspar, which proves on microscopical examination to be microcline with thin, irregular laminæ of albite intergrown nearly parallel to the macropinacoid. This microcline is sometimes wholly altered to pure white, coarsely foliate kaolin, which is impregnated with fine particles of galena.

Smaller and apparently wholly irregular parts of the vein are occupied on the one hand by quartz containing zircon in great abundance, quite equally distributed through it, and on the other by a mass of mixed fluorides. The quartz containing zircon (see this Journal, October, 1882, page 284), is always sharply defined from the massive white quartz of the main vein which is wholly without included minerals. The zircon crystals are imbedded largely in the quartz itself and to a less degree in compact, white kaolinite, a greenish yellow mica, and purple or green fluorite. These three minerals fill small irreg-

ular spaces in the quartz, and probably replace feldspar, for they are also found on fissure planes as distinct secondary deposits.

The kaolinite is usually very compact, but crystals were found in some small cavities. These are exceedingly thin, colorless, transparent, rhombic leaves, the acute angle being usually evenly truncated, producing sometimes an almost perfect hexagon. The result of many measurements under the microscope seems to indicate that the obtuse angle of the rhomb is slightly less than 120°, the best results varying from 118° 30′, to 119° 30′. The thinnest leaves show distinct action on polarized light, and extinguish parallel to the diagonals of the rhomb. The thicker crystals are made up of many thin ones which are usually not perfectly coincident in position, and sometimes form more or less perfect rosettes.

The mica occurs in masses or foliated upon fissure planes, and no crystals have been found. The kaolinite possesses the composition I, and the mica II, as given below. The kaolinite contained a small amount of fluorite in almost microscopic crystals, the quantity being calculated from the Ca found.

I.		II.	
SiO_2	45·93	SiO_2	52·59
Al_2O_3	39·65	Al_2O_3	29·72
H_2O	13·77	Fe_2O_3	1·40
CaF_2	0·84	CaO	0·26
		MgO	2·12
		K_2O	8·33
		Na_2O	0·50
		H_2O	4·39
	100·19		99·31*

The fluorides seem to fill the small irregular spaces left after the decomposition of the zircon-bearing quartz, the thickness of the fluoride mass varying from a thin seam to possibly two or three feet. They are always sharply defined against both pure and zircon-bearing quartz, the contact surfaces of the former being sometimes apparently huge crystals.

The greater part of the fluoride mass is now dull white and very compact, and is evidently made up of a mixture of at least two substances, neither of which has as yet been identified. Only in a single specimen is there any clue to the original mineral, but in this there is a small, solid mass of unmistakable cryolite with alteration to pachnolite (?) progressing upon its cleavage planes in the exact manner described in vein A, but passing into the compact white mixture already mentioned.

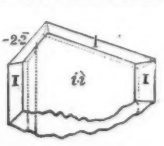
Adjoining the quartz is usually an irregular zone of purplish or greenish fluorite, and next to this a rather coarsely granular

* The presence or absence of fluorine was not ascertained.

mass of a colorless mineral with two distinct cleavage planes which passes gradually into the compact white substance. This zonal arrangement is not without exceptions, for both granular and compact masses come directly in contact with the quartz in some specimens, and fluorite is more or less abundantly sprinkled through the other substances; in fact the relation of the minerals to each other is such as to indicate that they are but different phases of alteration from a common source. The granular mineral occurs in sufficient purity to afford material for chemical analysis, and its individuals are large enough to admit of the preparation of thin sections with definite relations to the cleavage planes. The analysis first proved the identity of this mineral with prosopite, the optical properties shown by the thin section confirmed this determination and quite recently a few minute crystals were found in one specimen which agree with the published data on the Saxon mineral. As the identification of this rare species, particularly in its present association, is a matter of considerable interest, we will describe it somewhat in detail.

Prosopite.

Crystalline form and physical properties.—The minute crystals found in a single specimen from vein B as well as those observed on pachnolite, etc., in vein A (p. 288) are all of the habit shown in the accompanying figure. This is from a camera lucida drawing of a crystal measuring $0{\cdot}5^{mm}$ normal to the edge of the prism, and can therefore make no pretensions to crystallographical accuracy. The crystals are colorless and transparent, have uniformly a tabular form through the development of i-$\bar{i}$ (010) and show plainly the prism and two pyramids which may be considered as 1 ($\bar{1}11$) and $-2{\cdot}\bar{2}$ (211) for extinction takes place nearly or quite parallel to the edge of $-2{\cdot}\bar{2}$, which is, according to DesCloizeaux* and Groth,† the position of the bisectrix.



Thin sections prepared as nearly as possible perpendicular to the edge of the two cleavage faces in the irregular granular individuals, show that the angle of the cleavage planes is very nearly 135°, and that extinction takes place parallel to the direction bisecting that angle. This behavior agrees perfectly with the statements concerning prosopite, according to which the chief cleavage is parallel to $-2{\cdot}\bar{2}$, the angle of which is about 134°. The present material does not allow of a definite settlement of the question of the crystalline form of prosopite,

* Bull. Soc. Min. de Fr., v, 317. † l. c., p. 290.

but nothing observed is in conflict with the reference to the monoclinic system.

Chemical investigation.—The formula deduced by Brandl* for prosopite, from the results of his analysis as here given:—

		Atomic values.
F	35·01	1·842
Al	23·37	0·853
Ca	16·19	0·405
Mg	0·11	0·004
Na	0·33	0·014
H_2O	12·41	0·689
Loss as oxygen,	12·58	0·786
	100·00	

is CaF_2, $2Al(F, OH)_3$. The whole of the water is assumed to be basic, entering with oxygen into the constitution of the mineral as hydroxyl, the latter replacing an equivalent amount of fluorine. In support of this assumption, Brandl mentions that no loss is perceptible below 260° C.

In an earlier partial analysis, Scheerer (Pogg. Ann., ci, p. 361) found

Al 22·77, Ca 16·41, H_2O 15·50

Of the analyses tabulated below, those under I, II, III and IV were made upon material from vein B. That analyzed under I, *a* and *b*, was composed of comparatively large irregular crystalline pieces, showing no visible impurity whatever, having a sp. grav. at 23° C. of 2·880 and a hardness of about 4·5. As the ratio Al : Ca differed materially from that of 2 : 1 required by Brandl's formula, it appeared that some foreign matter must be present, consequently no further determinations were made, as it was hoped better material might be obtained.

Analysis II was made upon material separated from quartz, zircon, fluorite and other accompanying minerals by a solution of iodide of mercury in iodide of potassium. The result was a slight improvement upon the previous and the analysis was completed. The material for III was picked out carefully by hand with the aid of the loupe, but as the result was still not satisfactory, a further portion (IV) aggregating however only 0·1022 gr. was selected with the greatest possible care, every particle being distinctly crystalline and showing under the microscope no trace of impurity. Here a slight improvement becomes evident in the ratio, but as the amount taken for analysis was so extremely small, it cannot be asserted that the better results may not be due to unavoidable errors of analysis. The material for analysis V was derived from vein A, where the prosopite occurred on astrophyllite associated with cryolite, pachnolite, etc. This material was, however, evidently not

* Ann. de Chem., ccxiii, p. 13.

quite pure, being opaque and very slightly colored in part by oxide of iron. The analysis was made merely to prove its identity with the prosopite from vein B.

	I.		II.	III.	IV.	V.
	a	*b*				
Al	22·09	21·83	22·03	22·28	22·63	21·79
Ca	17·67	17·87	16·92	17·14	16·80	16·84
Mg	----	----	0·20	----	0·15	0·35
Na	----	----	0·48	----	0·48	0·79
Ka	----	----	----	----	----	0·11
H_2O	----	----	13·54 (13·37)	----	----	----
F	----	----	33·14	33.22	----	32·30
			86·31			
	Loss as oxygen,		13·69			
			100·00			

The mean of all the results under analyses I–IV is as follows:

Al	22·17
Ca	17·28
Mg	0·17
Na	0·48
H_2O	13·46
F	33·18
	86·74
Loss as oxygen,	13·26
	100·00

After subtracting from the fluorine an equivalent for the calcium, magnesium and sodium, and combining the remainder with aluminium, there remains of the latter 14·44 per cent, requiring 12·65 per cent oxygen, instead of 13·26 per cent found by difference. The atomic values appear as follows, after substituting for magnesium and sodium, an equivalent of calcium:

Al	22·17 ÷ 27·4	0·809		
Ca	17·98 ÷ 40·0	0·449		
F	33·18 ÷ 19·	1·746		
H_2O	13·46 ÷ 18·	0·748	1·539	3·285
O	12·65 ÷ 16·	0·791		

The result is unsatisfactory, the ratio of Ca : Al being as 1 : 1·78 instead of 1 : 2. In none of the material analyzed was the slightest trace of kaolinization to be observed, nor any other foreign matter. It therefore becomes impossible to explain with any degree of certainty the above abnormal results.

The general agreement of all the analyses, the aluminium being found too low and the calcium too high in each case, shows pretty conclusively that an explanation cannot be sought for in analytical errors.

As fluorite occurs here always in most intimate association with prosopite, and the possibility suggested itself that some of this might be so intergrown with the latter as to escape the closest scrutiny, it became desirable to ascertain what change

would be effected in the ratio above given by subtracting enough calcium to make the ratio Ca : Al as 1 : 2 and an equivalent amount of fluorine. The atomic values then become:

Al	0·809		2·00
Ca	0·404		1·00
F	1·656	3·195	7·91
HO	1·539		

which agree quite as well for the formula $CaAl_2(F, OH)_8$ as those obtained by Brandl.

If instead of the mean of all the analyses, the figures of II alone are taken for calculations similar to the above, the result is the same, even a little more closely approximating to the ratio 2 : 1 : 8.

The observation made by Brandl, that below 260° C. no loss in weight occurs, was found to apply here, provided the exposure to this degree of temperature is short. If continued for many hours a slight but sensible loss is observed.

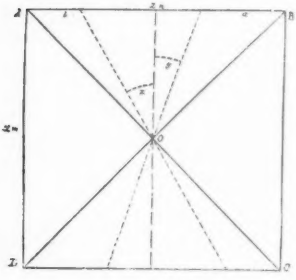
Denver, Colorado, June, 1883.

ART. XXXI.—*On the Origin and Hade of Normal Faults;* by W. J. McGEE.*

FAULTS may be either *normal* or *reversed;* i. e., may hade to either down-throw or up-throw. Both classes are probably developed by movements relieving radial strain (either simple or differential), tangential thrust, tangential tension, or a combination of these stresses, in the terrestrial crust. Only those due to radial strain in its essential combinations are here considered.

Let contiguous areas of a homogeneous rigid tract resting on a mobile sub-stratum (which may be either the gaseous, fluid or *quasi*-solid terrestrial nucleus, or a yielding layer any where within the exterior shell) be in unstable equilibrium, such as might arise from transference of sediment, and let all initial stresses save those immediately due to the disturbed equilibrium be at first supposed eliminated. In such case the tract will suffer differential radial strain culminating in the plane of coincidence of the unstable areas.

1.



* Read before the Iowa Academy of Science, May 31st, 1883.

Now manifestly, before fracture supervene the strain will be distributed over a considerable horizontal zone, and will be propagated vertically through the tract; whence the resultant of stresses at any point in the plane of imminent faulting will bisect a parallelogram similar to that whose base and perpendicular are respectively the width of the stressed zone and the thickness of the homogeneous tract. Make such parallelogram equilateral; denote base and perpendicular by $2n$ and $2m$ respectively; and let BC be the depressed side. The strain will then be resolved into stresses of opposite sign acting in the directions of the diagonals.

If now fracture relieve tensile strain only, it will occur at right angles to the tension-diagonal AC, or in BD; and the fault will hade to the up-throw. Such indeed is the direction actually taken by ice-stream crevasses (to which the diagram will equally apply) as shown by Hopkins;* for in slowly moving ice the crushing strength must be nearly *nil* and the compressive-elasticity infinite, while tensile stress probably increases rigidity and brittleness, and thus diminishes the tensile elasticity.† And were the antithesis of ice existent its fracture under the like stresses would occur at right angles to the compression-diagonal, or in AC, and its fault would hade to the down-throw.

But the materials of faulted tracts possess both tensile and crushing strengths, and also tensile and compressive elasticities. Denoting these in their order by t, c, t' and c', the values $t \times t'$ and $c \times c'$ are obtained for the moments of fracture orthogonally to the diagonals AC and BD respectively; which values may be represented by a and b, whose sum is proportional to $2n$. Obviously, if a and b be equal, the fracture will bisect the angle of the diagonals in nO; but if a be in defect (as in crystalline solids generally) the hade will ever be to the up-throw in the angle y, whose tangent is $(n-a)m$. From this expression the hade of fault in any greatly attenuated tract of determinate stress-parallelogram and known strength and elasticity might be readily computed.

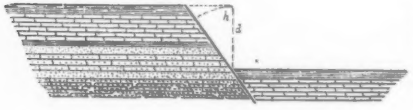
If the case be modified by the introduction of initial tangential stress, such stress will operate through mO with intensity i, and tend to produce fracture in nO. If the stress be tensile it must be less than a, and will at once relieve BD and strain AC proportionally to its intensity by the cosine of the angle formed by its direction and that of the latter diagonal, and thus (denoting such angle by θ) reduce a by $i \cos \theta n$; when the expression for hade will become $\tan y = (n - a - \overline{i \cos \theta n})m$,

* *Trans. Camb. Phil. Soc.*, viii, (pt. ii, 1844, 163–7.)

† *Vide* "On Maximum Synchronous Glaciation, III, 1, 2; *Proc. A. A. A. S.*, xxix, 1880, 497.

and the hade will ever diverge from the normal. Conversely, if the stress be compressive the relation will become $\tan y = (n-a+i\cos\theta n)m$ or $\tan x=(n-b-i\cos\theta n)m$ as the case may be, and the hade will ever approach the normal; but if it become normal the tendency to movement will be counteracted and a disposition to produce dragged fault or flexure will be engendered; since the development of normal faults demands that the lateral extent of the faulted tract shall increase from T to $T+(\tan h\times d)$ where h is angle of hade and d the vertical displacement.

2.



(The case in which tangential thrust is predominant, and the tendency hence toward ordinary reversed faults, complex folding and tumefaction, falls beyond the scope of this discussion.)

It thus appears that the general disposition of differential radial strain, either singly or combined with tangential strains, is to form reversed faults.

Let the case be farther modified by the introduction of simple vertical stress apart from those due to the unstable equilibrium. Were the stress tensile, its effect would be analogous to that of tangential tension; but since general vertical tension without predominant tangential thrust is inconceivable (whence the sum of tensile vertical stresses is always in defect), and since local vertical tension must equally react on adjacent *couches*, the effect of such stress is nugatory. But if the stress be compressive it will equally relieve AC and strain BD, and thus diminish b; and since within the crushing strength of the material of the tract there is no limit to this stress, b will be reduced to $b-v\cos\theta m$, where v is such vertical stress, and the original expression for hade will become $\tan x=(n-b-v\cos\theta m)m$; when the fault may hade strongly to the down-throw.

The natural case favorable to profound faulting arises in such an unstable tract as has been assumed within which exist vertical stresses due to its own weight and sufficient initial tension to admit of free lateral movement as rupture occurs. In such case the strain in BD and the disposition to give way in AC will progressively increase downward until the crushing strength of the material is reached; whence *the fault due to radial strain must originate with normal hade in a deep-seated hypogeal couche, and* (since rupture in each *couche* will throw the strain on the next higher) *must be propagated upward with progressively diminishing hade,* which may eventually become nothing or even reversed; *while below the couche of origin the fracture*

must give place to plastic flexure. The full expression for moment of fracture and angle of hade in this case is tan $x = \frac{}{(n-b-v\cos\theta m+i\cos\theta n)m}$; and since the several factors may be approximately evaluated it follows that the regimen of faults formed under given conditions, and conversely the conditions giving rise to faults of known regimen, are roughly determinate.

Below the couche of origin the substance of the tract must be in the condition of ice in a Brahma press or of lead in a pipe machine; and for the purposes of the present discussion, as for the purposes of physical geology generally, it is immaterial whether the mobility be molar or molecular. Obviously such substratum would be equivalent in effect to the *quasi*-solid terrestrial nucleus assumed at the outset.

If the unstable equilibrium were due to transference of sediment, the sub-surface strata would be compressed beneath the loaded area and allowed to expand under the lightened area, and the throw, as measured by corresponding strata, would consequently increase toward the surface.

In profound faulting, such as is here considered, minor departures of the plane of fracture from the plane of maximum stress may be neglected, since (1) the irregularities would be relatively slight, and since (2) incipient movement would tend to eliminate all such irregularities.

Fractures of the origin and character here contemplated may be denominated *Normal Faults of the First Order.*

Let a homogeneous rigid tract be subjected to simple vertical compressive stress due to its own weight. Within the limits of its elasticity, resulting vertical compression will be relieved by lateral extension; but beyond such limits lateral stress will be in defect, and fracture may occur in each prism of the stratum of imminent crushing just as if it were independent and unsupported laterally. Now as shown by Hodgkinson,* fracture in crushed prisms of crystalline texture occurs diagonally to the direction of stress and separates the prism into wedge-shaped segments; whence the stratum of incipient crushing may develop a series of diagonal fractures each bisecting a prism of height equal to the thickness of the stratum and of width determined by the constitution and texture of the material fractured. Only so much movement of the pyramidal segments can occur as will serve to equalize vertical and lateral stresses: the hade will ever be normal; and from the center of each plane of fracture the throw will progressively increase upward and downward to the apices of the contiguous segments. Such may be termed *Normal Faults of the second order.*

* Cited by Rankine, "Civil Engineering," 4th ed., 1865, 235.

Let a homogeneous rigid tract resting on a mobile substratum, or a tract consisting of alternate rigid and yielding strata, be subjected to vertical compressive stress due to its own weight; and let the rigid portion be traversed by oblique planes of fracture or of weakness due to any cause. Each rigid pyramid, prism or wedge bounded by such planes will then seek a hydrostatic equilibrium and will sink into or be upraised by the mobile substratum to a degree determined by the area of its base and its mass, and the initial planes of fracture or weakness will become faults of normal hade. As in faults of the first order, beneath the *couche* in which pressure equals crushing strength fracture will give place to flexure; and, since the behavior of any yielding stratum will simulate that of the mobile substratum, if there be alternations of rigid and yielding strata, or if all strata be imperfectly rigid, the throw will increase toward the surface.

Dislocations of this character may be designated *Normal Faults of the Third Order*. The principles involved in their development were long ago pointed out by Hopkins,* and are now too generally recognized to require thorough analysis.

Except in the last case the influence of joints in determining the regimen of faults may be neglected; for joints are known to be superficial, while faults are deep-seated.

Since the movements contemplated in the second case must be confined to strata of limited thickness, while in the third case the assumption of profound initial fractures or planes of weakness is unwarranted, the grander faults of the globe must be referred to other causes; and since differential radial strain is an adequate cause, while the phenomena of profound normal faults are collectively such as differential radial strain tends to produce, the hypothesis that these faults are due to such strains is warranted, and is here enounced.

If the hypothesis be valid, it will afford the means of coördinating faults and flexures; it will at once sustain the suggestion of Gilbert† that Appalachian flexures and Cordilleran fractures are but diverse manifestations of identical movements, and render the amount of denudation which the former region has suffered roughly determinate; it will go far toward demonstrating that, whatever be the condition of its interior, the terrestrial crust is—as already indicated by Dutton‡—in hydrostatic equilibrium; and it will afford a new vantage-point from which many of the complex problems of orology and terrestrial physics may be approached.

Salt Lake City, Utah, March 23d, 1883.

* "Researches in Physical Geology," *Phil. Trans. Roy. Soc.*, 1842, i, 53.

† "U. S. Geog. and Geol. Surveys West of the 100th Meridian," III, Geology, 1875, 62.

‡ Notice of Fisher's "Physics of the Earth's Crust," this Journal, xxiii, 1882, 288.

ART. XXXII.—*On the Sensitiveness of the Eye to Slight Differences of Color;* by BENJAMIN OSGOOD PEIRCE, JR.

AUBERT has shown * by his experiments with revolving discs that the eye is able to detect the change produced by the addition of 1 part of white light to 360 parts of colored light, and that perceptible changes in hue can be brought about by adding to light of any color less than 1 per cent of light of a different color. He infers from this that a normal eye could distinguish at least one thousand different hues in the solar spectrum.

At the suggestion of Professor Wolcott Gibbs, I have made a few experiments to test the sensitiveness of the eyes of different people to slight changes of wave-length in different parts of the spectrum.

For this purpose, a long, thin sheet of vulcanite was inserted lengthwise into the collimator of a large spectroscope so as to divide the tube into an upper and a lower half. The lower part of the tube received light from a fixed slit, the upper part from a movable slit of the same width, which could be set exactly over the other, or displaced to the right or to the left. The amount of displacement was determined by means of a steel scale fastened to the upper slit and moving with it past a fixed zero point. The light from the collimator fell upon a Rutherfurd diffraction-grating of about 17,000 lines to the inch, and the resulting spectra were then thrown, one above the other, into the observing telescope. A blackened, metallic diaphragm, out of which two narrow slits had been cut in the same vertical line, was placed in the eye-piece of the telescope, so that when the two collimator slits were even the observer saw merely two narrow strips (one over the other) of the same colored light on a black field. When the movable collimator slit was displaced, the *color* of the observer's lower strip was changed without changing its *position in the field*, and the object of the experiments was to see how small a displacement could be infallibly *detected* and *named in direction* by the observer.

The width of the collimator slit was about $\cdot 25^{mm}$, and the slit in the eye-piece diaphragm was nearly of the apparent size of the collimator slits as seen through the telescope. The length of that part of the spectrum which was so bright as to be easily studied (say from Li *a* to G) extended over more than 12 degrees of arc as measured in the instrument, and the width of the strip seen by the observer was about 5 minutes of arc, so that not more than the $\frac{1}{150}$ part of this brighter part of the spectrum was in the field at the same time.

* "Physiologie der Netzhaut," Breslau, 1865, pp. 132–154; Rood, "Modern Chromatics," pp. 39–41.

In no case was the observer able to detect any difference between the colors of the two edges of a strip; each strip seemed to be of one color throughout.

It was necessary to regulate very carefully the Argand burner used as a source of light, for the least difference in the intensities of the two spectra made the strip of the brighter seem to the observer yellower than the corresponding strip of the other spectrum. This error, due to the "color of brightness,"* was most apparent in observations made at the extremities of the spectrum, i. e. in the red and the violet.

A displacement of the movable collimator slit amounting to 1 scale division corresponded to 4 minutes of arc, or to a difference in wave-length of ·0000013^{mm}.

The observer sat in a darkened room and directed the telescope to that part of the spectrum in which the observations were to be made. After an assistant had placed the movable collimator slit even with the other, and the observer had seen to it that under these circumstances the strips of light in the eye-piece seemed just alike—as they should—the observer took his eye away from the instrument, and the assistant displaced the movable collimator slit and asked the observer to look into the telescope and to tell him, from the colors of the strips, in which direction the slit must be moved in order to be even again with the fixed slit. If the displacement was very large, the observer was able to tell immediately how to move the slit in order to match the colors of the strips; if in successive trials the displacements were made smaller and smaller, a place was reached where the colors of the strips did not seem well matched to the observer, but where he could no longer say with confidence *in which direction* the movable slit had been displaced.

The smallest displacements which the observer could infallibly detect and name were determined at different places in the spectrum, and it soon appeared, as was to have been expected, that the magnitude of these displacements was very different with the same person for different colors and not by any means alike with different persons for the same color.

Though the results seemed at first somewhat irregular, I found that the same series of results were invariably obtained with the same person, even though the experiments were separated by days or weeks, and that certain features were common to all the series obtained with the different observers.

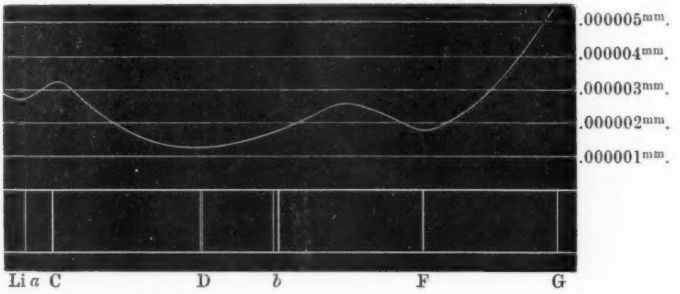
The largest displacement needed (except at the ends of the spectrum) by any of the observers corresponded to a difference in wave-length of the middle of the two strips of about ·000005^{mm}, and the smallest displacement which any observer could infal-

* C. S. Peirce, "Note on the Sensation of Colors," this Journal, April, 1877.

libly detect corresponded to a difference in wave-length of only ·0000005mm. This last is rather remarkable, for although no one could see any difference between the colors of the edges of either of the strips, a displacement which made the color of the middle of one strip the same as that of an edge of the other sometimes made the two strips distinguishable.

There was a singular uniformity in the performance of different eyes as judged by the average of these displacements for the whole visible spectrum. Perhaps smaller differences might have been detected if the strips had been narrower, but the work proved to be so trying to the eyes that I thought it best not to experiment further.

From a series of observations made by a number of different persons and extending over several months, two or three general conclusions may be drawn. To make these evident, I have plotted a curve by laying off over different places in the spectrum ordinates obtained by averaging in each case the least



displacements distinguishable by the different observers at that point. The general features of this curve appeared in the results of every observer.

In all cases the eye was most sensitive to changes in a color slightly less refrangible than that of the sodium line, though this color varied somewhat with different persons, being in some cases more orange and in others more yellow. In all cases the eye was more sensitive to changes in the color corresponding to the F line than to changes in colors lying half-way between *b* and F.

In many cases, though not in all, the eye was less sensitive to changes in a red near the C line than to a somewhat darker red beyond the Lithium line. In the darker colors at the ends of the spectrum it was, of course, very hard to distinguish small differences.

In addition to these general features there were in most of the curves obtained by plotting the results of the different observers lesser maxima and minima which showed peculiarities

in the different eyes. These peculiarities appeared unchanged in every set of observations taken by the same person.

I wish to express my obligations to Professor Gibbs for his advice and for the loan of apparatus, to Professor J. P. Cooke, who kindly placed at my service the magnificent spectroscope made for him by the Clarks, and, among others, to Professor W. E. Byerly, Professor C. R. Lanman and Messrs. F. B. Knapp, E. B. Lefavour and E. S. Sheldon for their kindness in helping me with their observations.

Cambridge, Aug., 1883.

ART. XXXIII.—*Injury sustained by the Eye of a Trilobite at the time of the Moulting of the Shell;* by CHARLES D. WALCOTT.

MR. WILLIAM P. RUST, of Trenton Falls, N. Y., called my attention some time since to the eyes of a small but very perfect specimen of *Illænus crassicauda*, from the Trenton limestone, that he has in his beautiful collection of Trenton fossils.

The left eye is perfect; the visual surface is clearly defined, and in the sunlight almost translucent between the darker base and the curve of the facial suture above. The right eye at first sight appears to have been broken in working away the matrix, but a close examination shows, as Mr. Rust expressed it, that the eye had been put out while the animal was living. This is shown by the peculiar growth of the shell about the aperture formerly occupied by the visual surface of the eye. The margins are turned in, rounded and contracted, and the size of the palpebral lobe materially lessened. An injury to the visual surface would scarcely produce this effect if the shell was hard. If slightly injured before the moulting of the shell, the separation would be imperfect and the visual surface carried away with the old shell would leave a cavity around which the new shell would form as in the eye before us. If injured before the new shell had hardened, that effect might be produced, but the probabilities are that the loss of the visual surface occurred at the time of the moulting of the old shell.

Among the thousands of trilobites that have passed through my hands in which the eyes were preserved, I have never noticed any distortion or injury that occurred during the life of the animal. In a few instances, the shell of the pygidium of *Asaphus platycephalus* has shown evidence of local fracture that appears to have occurred during the life of the animal, but these were very unsatisfactory. To Mr. Rust's skill in working out the specimen described and also in detecting the character of the injured eye, we are indebted for some positive information of an injury sustained during the moulting of the shell of a trilobite.

ART. XXXIV.—*Dip of the Rocks in Central New York;* by Professor S. G. WILLIAMS.

FROM a careful study of the outcrops of the Tully limestone over an extent of more than eighty miles east and west, in the counties of Yates, Seneca, Tompkins, Cayuga, Cortland, Onondaga and Madison, data have been obtained for an approximate determination of the dip of the strata in that part of the state. This limestone, from its numerous bold outcrops in the nearly meridional valleys of the lakes and streams of the counties named above, forms a very convenient stratum for this purpose; whilst the elevation of the lakes of central New York above mean tide, and, beyond the region of the lakes, the best attainable railroad levels, have given the means of obtaining approximately the height of this great sheet of rocks above the sea level, at many important points, by observations with the aneroid barometer, often several times repeated and verified by readings at many stations.

The height of the north outcrop of the Tully between Seneca and Cayuga Lakes has been determined, by levelling, to be 739 feet above the sea, or 292 feet above Seneca Lake. From this point S.W. to the sharp synclinal bend figured in Hall's Report on the 4th District, at page 212, is four miles, in which the strata descend 252 feet or 63 feet per mile; or if we disregard this sharp local bend and two other smaller ones farther south, and extend the observation to the point of final disappearance in the town of Lodi, a distance of 7·8 miles 25° west of south, the dip is 37½ feet per mile. From this same point, which is 362 feet above Cayuga Lake, to Little Point on that lake, where the Tully is carried below the lake level by a synclinal bend and remains submerged for four and a fourth miles, is a distance of about ten miles S. 42° E.; the dip is therefore 36 feet per mile. Little Point is 4·73 miles S. 10° W. from King's Ferry on the east side of Cayuga Lake, where the Tully is 225 feet above the lake, giving a dip in this case of 47½ feet per mile.

From King's Ferry to Lake Ridge on the east side of Cayuga Lake, a distance of a little more than 4·4 miles S. 13° E., the strata descend 190 feet, a dip of 42½ feet per mile, and observations extended northward from King's Ferry show that this rate of dip holds good for more than six miles farther. From these data and others of the same purport, I judge that the dip of the strata in the Cayuga Lake region, north of the bend immediately to be mentioned, is at least 45 feet to the mile and in a direction probably about ten degrees west of south.

On the west side of Cayuga Lake south of Little Point, after

the Tully has been submerged for more than four miles, it rises again from the lake and makes an arch of 4·4 miles span and 160 feet in extreme height, descending to the south at the rate of 80 feet to the mile and northward 67 feet per mile. On the opposite side of the lake, the limestone immediately north of the arch is nowhere fully submerged, but after continuing nearly horizontal for about three miles, rises into an arch of over six miles span, the highest observed point of which is 235 feet above the lake. From this point the dip to the south, which is much the most rapid, is 110 feet per mile. In his Report on the Third District of New York, p. 167, Vanuxem attempts to show that this arch is not a real upward bend of the strata but only an apparent one caused by a bend of Cayuga Lake to the southeastward. It is a sufficient answer to this theory to state that on the west side of the lake, the outcrops of the Tully, when laid down on an accurate map, for more than nine miles, including the whole disturbed region, do not deviate five degrees from a straight line. A line drawn through the lowest points of the synclinal on Cayuga Lake passes through the sharp synclinal bend on Seneca Lake before alluded to, and strikes the outlet of Crooked Lake near a point where the Tully has apparently suffered a fault of about a hundred feet. This line has a direction of about W. 27° N.

Disturbances similar to those on Cayuga Lake, but less in extent, occur on both sides of Seneca Lake, involving, however, in the vicinity of Lodi, two subordinate bends of a few feet each, which appear on both sides of the lake.

The northernmost observed outcrop of the Tully in the Owasco Lake valley is a mile west of Scipio station on Owasco Lake. This outcrop is 1110 feet above the sea, from which, to the lowest point of the synclinal on Cayuga Lake, there is a descent of 730 feet in about seventeen miles, or 43 feet per mile southwesterly. From the same point southeastward to the most southern exposure in the Owasco valley, there is a descent of 310 feet in nine miles, or 34 feet per mile. Some not very reliable data, extending over a space of nine miles between Owasco and Skaneateles Lakes, show an apparent dip of at least 40 feet per mile S.S.W, but there is good reason for believing that this is less than the reality. On Skaneateles Lake the dip of the Tully from its north outcrop near Borodino, Onondaga county, to Glen Haven, at the heed of the lake, is 210 feet in five and a half miles, or 38 feet per mile a little east of south. Hence there is no good reason for thinking that in the valleys of Owasco and Skaneateles Lakes the dip of the rocks varies materially from that in the Cayuga Lake region.

East of Skaneateles Lake but three useful points have yet been obtained and these only approximately. One of these,

Pickett's Hill, in Fabius, Onondaga county, shows the Tully about 1680 feet above the sea. A second station, over a mile east of DeRuyter village, Madison county, is distant from the first six and a half miles in a southeasterly direction and is about 260 feet lower, a dip of 40 feet per mile. The third station, near the village of Cuyler, in Cortland county, is also six and a half miles from the first, a little west of south, and is about 365 feet lower, being a dip of 56 feet per mile. Hence it would seem that the dip of the strata increases somewhat toward the east, and is there also directed a little west of south.

The distances given in this article for the Cayuga Lake region were obtained by scale from an accurate map of Cayuga Lake made by the Engineer Department of Cornell University, and are doubtless correct. The remaining distances were taken by scale from Asher and Adams' Atlas of New York, verified by comparison with a large map of New York and with the best attainable county maps, and can be considered only as reasonable approximations; still the substantial agreement of results from data derived from several different sources makes me confident that they may be relied on as reasonable approximations to the dip of the strata over the region in question.

Cornell University, Ithaca, N. Y., Aug. 3.

ART. XXXV.—*Physiological Optics;* by THOMAS WILLIAM BACKHOUSE.

THERE are some points in Mr. W. LeConte-Stevens's papers, published in this Journal, to which I take exception, though other points I must confirm.

The papers go to disprove Brewster's theory that the mode of judgment of distance in binocular vision is by the optic angle, interpreted by means of triangulation. Some of Stevens's objections to this theory seem to me to be untenable. I gather that he regards LeConte's theory as insufficient, though he partly accepts it. I shall proceed to criticize some of Stevens's remarks.

Vol. xxiii, p. 291.—He discards the theory of visual lines and of corresponding retinal points, and substitutes that of associated muscular action. But if this be the true one, must it not *first* be derived from observation of the intersection of visual lines?

I understand the theory of corresponding points to be that the brain causes each point on one retina to appear to coincide with a point on the other, and that these coincident points are invariably the same. Stevens doubts this, though I do not

well understand how far his theory of "partially correspondent retinal areas"* differs from it. But even in this modified theory he considers "this correspondence must be considered merely the effect of association resulting from oft-repeated experience." But surely experience would be far more easily learned if based on some anatomical or mathematical fact than if there were no such basis to go upon; and therefore it seems to me that there must be such a basis, and that it is consequently very improbable that the theory of corresponding points can be incorrect. Nevertheless, the facts he gives in volume xxiv, page 241, etc., and page 331, etc., perhaps point in this direction showing, as they do, that the brain is often unconscious of double images, and also that stereoscopic relief is obtained when the time is too short to allow much play of the muscles upon different objects in view. Indeed, when spectral after-images are binocularly viewed there can be no play of the muscles. Is it possible, then, that we judge of distance by the brain uniting similar points of the two images, *even when they are not on corresponding points of the retinas*, and judging by the distance from the actual corresponding points? LeConte's theory† seems more probable, viz: that we judge partly by observing, *perhaps often unconsciously*, that objects are double, and more accurately by moving the point of sight backward and forward by the muscles, converging the eyes successively on the different distances in sight. I think that observations on persons who habitually squint would throw much light on these questions; but they are not easily made.

Vol. xxii, p. 444.—From the experiment on viewing parallel lines by divergent vision it looks as though the brain judged more of the *relative* distance of different objects than of *absolute* distance. Stevens suggests this in volume xxiii, page 356, lines 1 to 4. Likewise, geometrical forms viewed in a stereoscope do not give any idea of absolute distance but only of relative distance. I take this to be owing to the conflict of focal and axial adjustment.

Page 294, etc.—This paper shows that we can judge of distance pretty well when axial and focal adjustments agree, but not otherwise. This is not surprising when it is considered‡ that there must always be three elements that influence our judgment of distance, viz:

1. The optic angle, interpreted through the rectus muscles of the eye-balls.

2. The focal adjustment, interpreted through the ciliary muscles.

* Vol. xxiii, p. 359. † Vol. ii, p. 3.

‡ See p. 292 and vol. xxii, p. 360, though in the list in the latter place focal adjustment is somehow omitted.

3. The visual angle, or apparent size of the object.

In the experiments in the paper in question these three elements conflict with one another. It is therefore no argument against Brewster's theory to find that the judgment is at fault. For if the eyes, by means of triangulation of the object, say positively that it is at a certain distance (as in the experiments on page 294), while the ciliary muscles say equally positively that it is at another distance, how is the brain to decide? Unless it gives paramount weight to one mode of judgment it will naturally conclude that the true distance is somewhere between the two. In the experiment on pages 295 and 296 on the pattern combined by axial divergence,* there is, in addition, the judgment by the visual angle to contradict the other modes. This is really judgment by parallax of motion, so that the two modes of judging parallax clash—that by motion and that by triangulation (according to Brewster's theory.) It is not surprising that the former outweighs the latter. Brewster's theory is, therefore, not contradicted by the experiments. His theory is that the parallax of the object observed by the two eyes tells us its distance. In ordinary circumstances this must agree with the much larger parallax obtained by moving from one place to another; but in this experiment the two are contradictory, the brain naturally prefers to judge by the larger parallax.

Page 297.—The apparent curvature of the "phantom wall" may probably be explained in the same way. I have tried to do the experiment as far as possible, but as my eyes have not the elasticity of Stevens's (and probably LeConte's and Brewster's), I have not had much success; it is therefore only a supposition that these observers focussed their eyes upon the real wall. The effect of this would be that while the ciliary muscles would tell them the image was at the true distance of the wall, the rectus muscles (or the triangulation), would say that it was a great deal nearer, and therefore they would judge that it was really at an intermediate position. Now, while (as stated on page 297) "it is an obvious geometric necessity that the focus of the phantom surface must be a plane parallel to the" wall, this is only true as long as it is judged to be at the distance calculated by triangulation. If it is judged to be more distant, and the angle enclosed by the optic axes therefore smaller than it really is, it will be a geometric necessity that the surface will appear curved, convex to the eye.

The nearest approach I can make to this experiment is as follows: I hold a small object, say a thin stick, between my eyes and a wall paper, on which is a regular pattern, in such a

* When there is optic divergence, as it represents an impossibility, of course it must conflict with the focal adjustment.

way that the stick comes between one eye and one part of the pattern, and between the other eye and a corresponding part of the pattern. By looking at the stick a short while, especially if it is moved about in or near the plane parallel with the wall, the pattern is soon seen as a phantom at the exact distance from the eyes that it should be by triangulation, i. e., at the same distance as the stick is when placed as described above. The eyes are focussed on the stick, and therefore the phantom is very much out of focus; but, inasmuch as there is no dissociation between the focal and axial adjustments, the brain does not judge that the phantom is out of the focus but that the pattern is really of an indefinite character, with all its outlines softened and irregularities toned down. I assume that the difference between this experiment and that of Brewster, LeConte and Stevens lies in the focal adjustment. The result is that the wall generally appears to me perfectly flat.

In three of the observations, when the optic angle was between 34°·1 and 45°, I suspected a slight convexity of the phantom; indeed, in one case I thought it decided; but in another, when the optic angle was 31°, I at times suspected that it was slightly concave. In all other cases (up to an optic angle of 42°·9), it was perfectly flat. In some cases, usually with a large optic angle, there was a slight appearance of curvature in the straight lines on the wall, convex to the point of sight; but, on the other hand, in two cases I suspected they were concave to the point of sight. This appearance of curvature in the straight lines was sometimes connected with an appearance of curvature of surface, and sometimes with a flat surface. I cannot account for the illusion, which did not follow any perceptible law; it may perhaps have been caused by the patterns used.

The largest optic angle I could observe with was 45° (distance of phantom 3·04 inches). Stevens does not say how large an angle is necessary to produce the effect of a convex surface, so it may be that my want of any decided success is owing to not having compassed a large enough angle.

In these experiments the nearest point at which I could see an object in perfect focus was 4·5 inches (optic angle 31°·0), and if my theory is correct it may be that the suspicion of convexity with large optic angles is owing to the stick not being in focus; i. e., to its being impossible for me to obtain a focal adjustment corresponding to the axial adjustment, though it is very probable that the strain on the ciliary muscles in endeavoring to bring it into focus is sufficient to prevent the judgment of distance by means of the optic angle from being materially warped by that through the ciliary muscles.

If the eyes range about over the phantom wall and the stick

is also moved till it is apparently placed exactly in the plane of the phantom, I have found that it is then exactly parallel with the real wall. At least, in eight independent observations and measurements of its position, the maximum deviation from parallelism was only 1°10′, and the average deviation 30′, which is not more than would be expected from errors of observation.

After getting the eyes well accustomed to the phantom it is possible to observe it for some little time after taking the stick away (especially if the optic angle is not large), before the eyes relapse into the position for viewing the wall naturally; during this time the focus is apt to lengthen, but the phantom continues to appear flat.

Vol. xxiii, pp. 298–301.—These experiments are of quite a different character from that of the phantom wall, inasmuch as the optic axes are nearly parallel, thus essentially changing the conditions. For the brain is perfectly conscious of the difference, and judges accordingly. It is most unlikely that the curvature of the retina (p. 298),* has anything to do with the phenomenon, which is easily explained on page 299, without reference to it. But the explanation of the effect on the points F and G appears to be incorrect, the fact being that combination is effected not between corresponding points on the two circles but, since a circle always appears as an ellipse in perspective whatever way it is viewed, *different* points in the two are combined, viz: points having the *same* retinal latitude.

I have not access to Rogers's paper on the combination of points differing in latitude, but I apprehend this can be effected only by moving the optic axis of one eye higher up than that of the other, so that the images of the two points still fall on corresponding points of the retina. As I have just shown, the experiment here described does not bear on the subject, and therefore cannot in the least invalidate the theory of corresponding points.

Vol. xxiii, p. 357.—I have seen stereographs of the moon showing it in relief, but these are always obtained by combining two photographs taken at *different times* when the moon's face by its rotation has been brought into a different position. I have never seen a stereograph of the sun, but if Stevens's statement is correct I have no doubt that the one he alludes to was produced in the same way. I do not dispute Stevens's remark that a very minute difference between two pictures is sufficient to produce the effect of relief; but these statements about stereographs of the sun and moon, if not explained, are calculated to give an exaggerated idea of this.

Sunderland, July 11, 1883.

* And vol. xxiv, p. 243.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Possible Variability of the Law of Definite Proportions.*— In the July number of this Journal (page 63 of this volume), Professor Barker has given a very full summary of the views of Boutlerow on this subject, which have been recently presented to the Chemical Society of Paris by Wurtz, and emphatically endorsed in the discussion which followed by Schutzenberger. The opinion was then and there clearly expressed that the "chemical value" of a constant weight (or rather mass) of a substance might vary, and that the so-called atomic weight of an element might be simply the carrier of a certain amount of chemical energy which is variable within narrow limits; and further the question was asked whether "Prout's hypothesis may not be a true law which like that of Mariotte admits of a limited variation." Moreover numerous facts were cited, chiefly results of quantitative analysis, which seemed to confirm the views thus expressed. Such opinions are certainly very revolutionary, and, if they prevail, must entirely change the fundamental conceptions of chemical philosophy. Chemical combination can no longer be regarded as the juxtaposition of the definite invariable masses we call atoms, but must be considered as the "reciprocal saturation" or "interpenetration" of masses which may vary with the relative strength of their chemical energy acting at the time; and this change of the fundamental conception is inconsistent with the atomic theory and with the superstructure which modern chemistry has built upon it.

In the connection above referred to it is stated that Boutlerow had held the views then advanced for the past three years. Such views are, however, by no means new. They were not only expressed, but fully worked out by the writer more than twenty-five years ago. They were presented to the American Academy of Arts and Sciences in 1855 and fully developed in a paper published in the Memoirs of this Academy (New Series, vol. v, p. 337) "On Two New Crystalline Compounds of Zinc and Antimony and on the Cause of the Variation of Composition observed in their Crystals." The same subject was also discussed in a paper published in this Journal, "On an Apparent Perturbation of the Law of Definite Proportions observed in the Compounds of Zinc and Antimony" (Second Series, vol. xx, 1855). In these papers the opinions now under discussion were brought forward, not simply as speculations, but as a legitimate theory, which was advanced to explain the facts observed in a long and laborious investigation of the order of the variation in the composition of certain well-defined crystals, obtained by the author from alloys of zinc and antimony of different percentage composition by the "method of fusion." Alloys were crystallized differing by from one-half to five per cent,

according to circumstances, from the alloy containing 95 per cent of zinc to that containing 95 per cent of antimony, and analyses were made of the crystals thus obtained. Two well-marked types of crystals, and only two, were obtained corresponding to the composition Sb_2Zn_2 and Sb_2Zn_3; but it was found that while crystals of the first type might be made either with an excess of zinc or an excess of antimony, the crystals of the second type would only take an excess of zinc; and the whole order of the phenomena indicated—first, the action of a force tending to unite the metals in one of two definite proportions; secondly, the effect of the excess of one or the other metal in the alloy tending to disturb this result; thirdly, the interference of the two compounds with each other in alloys of intermediate composition. The results were plotted and a curve drawn showing the order of the variation, and this curve indicates plainly—first, the strong tendency of the chemical force, and secondly the disturbing effect of the excess of either metal in the alloy. Moreover, the manner in which these two influences modified each the other's action seemed to preclude the supposition that the results could be the effect of a simple mechanical enclosure by the growing crystals. For all these details we must refer to the papers cited. We will only quote a few sentences which indicate the drift of the writer's opinions at the time.

"In the absence of any known principle of chemical science by which the remarkable variations of composition that have been demonstrated in this memoir can be explained, the conclusion is almost forced upon us that zinc and antimony are capable of uniting and producing definite crystalline forms in other proportions than those of their chemical equivalents; in other words, that the law of definite proportions is not so absolute as has been hitherto supposed."

"Accepting the view of the subject that has been offered, it will be obvious that the very large extent of the variation in the compounds of zinc and antimony is due to the very weak affinity between these elements. Were the chemical force stronger in proportion to the disturbing force, the variation would be lessened; were it weaker, the variation would be increased."

"To what extent this perturbation of the law of definite proportions prevails among chemical compounds, it must remain for future investigation to determine. There are, however, a number of facts which tend to prove that it is very general whenever chemical affinity is weak." Examples are cited in the memoir.

"If variations in composition of such magnitude are possible when the force of chemical affinity is weak, it is highly probable that some variation may occur when the force is strong; and whatever view may be taken of the cause of the variation, it will now become a matter of importance to ascertain whether many discrepancies of analysis hitherto referred to imperfections in the process may not be owing to the same cause which influences the composition of the crystals of the antimonides of zinc.

"The definite proportions I regard as a maximum toward which the chemical force strives, a maximum from which the deviations in most cases are small, although in others they may be very large; and I maintain that this view of the subject, which the memoir has aimed to establish, is supported by the analogies of nature.

"I have shown in a former memoir that these numbers" (the equivalent numbers) "may be connected by a very simple numerical law, but here, as in other cases, we find merely a tendency towards the law, not an absolute agreement with it, the differences between the theoretical and the experimental equivalents being in many cases too great to be covered by errors of observation. The present memoir may throw light upon these discrepancies; for, to say the least, it is possible that the differences may originate in variations of the equivalent itself, and that the theoretical equivalent may be the maximum towards which the chemical force tends."

In a subsequent paper entitled "Crystalline Form not necessarily an Indication of Definite Chemical Composition, or on the Possible Variation of Constitution in a Mineral Species independent of the Phenomena of Isomorphism," published in the Philosophical Magazine (June, 1860), during a visit to London, the writer shows that whatever explanation may be given of the variations of composition, which he had previously described, such variations must necessarily modify the composition of many crystalline mineral products. Examples are cited in which this is obviously the case, and it was proposed to designate by the word "Allomerism" "a variation in the proportions of the constituents of a crystallized compound without any change in the crystalline form."

Now when after twenty-five years the same question is revived, although without any reference to previous work on the subject, and the same opinions are expressed under conditions more likely to give them currency, it becomes interesting to enquire whether the progress of chemistry during the interval has tended to confirm or invalidate these views. So far as the writer is aware no investigation, except his own already referred to, has been directed to the exact point in question, and in none of the analytical results which have been more recently cited as possible examples of variation in the combining proportions has it been conclusively shown that the discrepancies may not result from causes such as will be considered below.

During the last twenty-five years, however, a more extended knowledge has been acquired of that class of indefinite chemical compounds which like the alloys seem to occupy an intermediate position between definite composition and solution. Such compounds for example as many of the hydrates and silicates in which it is impossible to mark the limits between chemical and mechanical action. As in the case of the crystalline compounds of zinc and antimony, so in all such cases, some mechanical ac-

tion,—such as is manifested in what we call solvent power,—seems to modify the chemical force: but in no case except the first has any attempt been made to investigate the interaction of the two. The atomic theory explains such cases of indefinite combination by distinguishing between atomic and molecular union, and it regards the complex products as resulting from an aggregation of dissimilar molecules, in each of which, however, the law of definite proportion rigorously holds. It is necessary, however, in many cases to assume the existence of very complex molecules, and the doctrine of interpenetration certainly gives a simpler conception of this class of phenomena.

On the other hand the extension of our knowledge has either brought to light new facts or given prominence to old ones, which, by explaining anomalies, have tended to confirm the atomic theory. It has been found that crystals have a very remarkable power of enclosing foreign materials in their structure, so that at times the crystallizing material forms but a small portion of the resulting mass. If a crystal may enclose material wholly foreign to its composition, why may it not enclose an excess of either of its ingredients? There can be no doubt that in such cases the amount of material thus enclosed depends to a greater or less extent on the chemical force, and there is a continuity in the composition of the products which makes it difficult to distinguish between the material controlled by the chemical force and that which is not, and gives at least the appearance that the amount so controlled may be variable.

On the assumption, however, of the existence of molecules and atoms it is a very probable hypothesis, that when we bring together in solution or in fusion substances capable of yielding definite compounds, the number of molecules of such compounds that would form *at any one time* must depend—certainly among other conditions—on the strength of the chemical force as well as on the proportion of the several materials present. When, now, crystals form in such a menstruum we should expect that while the molecules of the definite compound would aggregate together, the crystalline structure thus formed would enclose, to a greater or less extent, molecules of one or the other of the constituents of this compound if present in excess, and the amount of such enclosure must depend on the relative proportion of the molecules in the menstruum; and this proportion, as we have seen, must have been already determined by the strength of the chemical force. Thus might arise such a regulated variation of composition as the writer's investigation indicated. The extent of the formation of molecules of definite composition in the menstruum would obviously be regulated by conditions, whose action would determine no break of continuity; but the segregation of these molecules in the formation of crystals would cause at once a sudden change. Crystallization is always a break of continuity, and although the crystalline structure may, as we have seen, enclose foreign material to a very large extent, yet, as the writer's investigation

proved, there is always, even in extreme cases, a tendency to exclude such material, a tendency which is the more effectual in proportion as the crystalline molecules preponderate in the menstruum. Hence it is that by repeated crystallization perfect purity may, in most cases at least, be secured. The hydrates of sulphuric acid furnish a good illustration of the theory we are discussing. Water and oil of vitriol may be mixed in any proportion and there is a perfect continuity between the various stages of dilution. When however on exposure to cold the definite hydrate $H_2SO_4.H_2O$ crystallizes out this continuity is abruptly broken, and it is reasonable to suppose that the molecules of this compound preëxisted in the liquid and only aggregated in the process of crystallization. These crystals may be obtained between quite wide limits of dilution, and must enclose more or less of the menstruum, and, therefore, vary in composition to a limited extent; but the definite hydrate can be obtained pure by repeated crystallization.

Another class of facts bearing on the same subject has been forced upon the writer's attention during the last few years by the investigations he has undertaken on the revision of the atomic weights. In a memoir on the "Numerical Relations between the Atomic Weights," which he communicated to the American Academy of Arts and Sciences, February 28th, 1854, he remarked —as the result of a careful discussion of the determinations which had been made up to that time—that the constant errors "seem to be the great errors in all these determinations, those accidental errors which were made in the repetitions of the same process by equally careful experimenters being comparatively insignificant." The writer's subsequent experience has entirely confirmed this opinion.

Of these constant errors by far the most important are those due to impurity of material, whether in the substance analyzed or in the products of the analysis. No problem of chemistry is more difficult than to prove that the materials and products involved in atomic weight determinations are perfectly pure, and represent a condition in which the elements are united in the definite proportions sought. It is of course very easy to discover such impurities as can be detected by well known analytical processes. But how can you be sure that a given oxide, sulphide, chloride, bromide or iodide does not contain a similar compound of a lower or higher order? How can you be sure that either of these compounds does not contain a small amount of oxide? It is such impurities as these that are the chief source of uncertainty, and they cause variations of composition precisely similar to those discussed above. By referring to the examples cited on page 65 of this volume it will be found that they are for the most part, if not all, open to a suspicion of errors of this kind, and until these suspicions are removed the evidence which the alleged discrepancies furnish is not trustworthy. As a rule, when a substance is formed by a simple reaction from perfectly pure

materials, the purity of the product is taken for granted, and yet the writer has shown that sulphide of antimony, when precipitated by hydric sulphide from tartaric acid solutions acidified by hydrochloric acid, invariably contains more or less oxichloride of antimony, which leaves its equivalent of oxide of antimony when the sulphide is dried at 200° C. So also he found it impracticable to prepare chloride of antimony free from oxide.

It may then well be asked what proof can we have of purity in such cases as have been discussed beyond the very definiteness of composition which is in question? Before attempting to answer this question the writer would call attention to the fact that constancy of analytical results affords no certain criterion of purity. In the writer's own experience constancy of results has frequently meant nothing but constancy of impurity, and the illusion caused by coincidences so-called has been forced again and again on the writer's attention. Hence no valuable conclusion can be reached by attempting to combine, according to the method of least squares, results of atomic weight determinations made by different chemical processes. The writer undertook such a discussion, as stated in his early memoir on the relations of the atomic weights, above referred to, but abandoned it simply because accidental errors are the only ones which such mathematical methods can eliminate and these were found to be quite insignificant factors in the problem. No useful conclusion can be deduced by comparing different classes of results, each affected by unknown constant errors; for it is impossible to assign to them correct values and in comparison with results from which constant errors have been eliminated they are all worthless. For example, in the writer's published work on the atomic weight of antimony, although a closely accurate result was obtained by the synthesis of the sulphide of antimony, this result is of no value in comparison with that obtained subsequently from the analysis of bromide of antimony, and to assign it any value in a discussion of probable errors would lead to error.

If now the question is still pressed: what is the criterion of a pure and definite compound? the only explicit answer that can be given is that a substance obtained by repeated crystallizations offers the most uniformly satisfactory evidence of purity that we possess. It is well known, however, that repeated crystallizations will not always ensure purity, and that very frequently the crystals, though pure when formed, cannot be collected and preserved without change. Moreover, it is seldom the case that such repeated crystallizations are possible. In most instances we must rely on other criteria of purity, such as constancy of physical properties like the melting or boiling points, and often the question can only be answered by a special, frequently a difficult, investigation. Indeed herein lie the great obstacles which we meet in all attempts to fix with accuracy the values of the atomic weights, and good judgment is the only criterion on which we can always depend.

It is surprising how few substances fulfill the conditions required in an accurate atomic weight determination, rarely more than one compound of each elementary substance; and for some of the most common elements, so far as we yet know, not even one. After the process has once been worked out, the actual analytical work is generally readily accomplished and the accidental errors involved are comparatively small; and when the constant errors have all been eliminated the definiteness of the results is surprising; and cannot but produce in the analyst a confidence in the definiteness of the law which determines such effects. With the writer, while his earlier work on crystalline alloys left an impression of the possible indefiniteness (under certain conditions) of chemical combination, his later work on atomic weights has shown him the exceedingly definite aspects of the same phenomena. Moreover, he has not found any evidence that the definiteness of chemical combination diminishes as the chemical force is weakened, a result we should expect if a variation were possible and as was predicted in the memoirs that have been so often cited. On the contrary, the writer has found that when purity is secured the proportions are as definite in weak compounds as in strong, in oxide of silver as in water; only with weak compounds it is usually more difficult to ensure purity.

While therefore the writer feels that the weight of evidence is at present in favor of the atomic theory, and of that absolute definiteness of combining proportions which this theory involves, still he is very glad that the whole question has been reöpened in such an authoritative way as is likely to lead to its solution. His own investigations, now in progress, will have a direct bearing upon it, and until the requisite facts have been established there is no sufficient ground for a positive opinion. But, although it must be admitted that the atomic theory is the only basis on which a consistent philosophy of chemistry can at present be built, the writer must confess that he is rather drawn to that view of nature which refers all differences between substances to dynamical causes, and which regards the atomic theory as only a temporary expedient for representing the facts of chemistry to the mind.

J. P. C.

2. *On the Spectrum of Beryllium.*—The position of beryllium among the elements still remains doubtful. Nilson and Petterson regard it as a triad with an atomic weight of 13·8 and a specific heat of 0·4079 to 0·4083, its oxide having the formula Be_2O_3. Emerson Reynolds, on the other hand, believes it to be a dyad, with an atomic weight of 9·2 and a specific heat of 0·642 (calculated), and assigns to the oxide the formula BeO. The specific heat, as determined experimentally by the former chemists, is somewhat uncertain since the metal employed contained from 5·59 to 13 per cent of impurities and was not homogeneous, being partly crystallized and partly in fused globules. To throw additional light upon this question HARTLEY has examined the spectrum of beryllium. If a triad, its spectrum should show consider-

able analogy with those of aluminum and indium; though if a dyad, the spectrum, in accordance with the periodic law, would not necessarily coincide with those of magnesium, zinc and cadmium, the other members of the homologous series. The spark spectrum of beryllium was obtained from a saturated solution of the chloride prepared from pure oxide. No lines belonging to other elements were seen in the spectrum except two very faint ones of calcium from the hydrochloric acid used. The spectra were produced with a prism and also with a Rutherfurd grating of 17,460 lines to the inch, and were photographed with a camera, the lens of which, as also that of the collimator tube, was 36 inches focus. Seven lines were observed, two of which were in the visible spectrum; but of these only one was photographed and that was too faint to be measured. Of the five lines photographed and measured, the third, of wave-length 3320·5, the fifth, 2649·4, the sixth, 2943·2, and the seventh, 2477·7, were strong and sharp, while the fourth, wave-length 3130·2, was very strong, still more extended and the most persistent of all. As a result of his comparisons the author says: "The spectrum of beryllium exhibits no marked analogy with the calcium, the magnesium or the aluminum spectra, all of which are members of well-defined homologous series. There is nothing similar to the boron, the silicon or carbon spectra, nor to those of scandium, yttrium or cerium. The spectrum of lithium is the one most allied to that of beryllium in the number, relative position and intensity of the lines. The character of the lines of greatest intensity in the beryllium spectrum is more like that of the pairs of lines in the spectrum of calcium, with wave-lengths 3933–3967·5 and 3705·5–3736·5. In fact the similarity to the line 3705·5 is so great that the two when juxtaposed on one plate may be mistaken if their positions be not taken into account. Lines of similar elements have very distinctive features. Individual lines of the calcium, strontium and barium group are quite different from those of the magnesium, zinc, cadmium series, and they could not be mistaken. The lines of cerium and didymium again are quite dissimilar in character; instead of being broad they are exceedingly fine. I am therefore led to the conclusion that beryllium is the first member of a dyad series of elements of which in all probability calcium, strontium and barium are homologues."—*J. Chem. Soc.*, xliii, 316, June, 1883. G. F. B.

3. *On a Relation between the Density and the Chemism of the Elements in different Allotropic states.*—MÜLLER-ERZBACH has called attention to the apparently necessary connection between the activity of a chemical reaction and the amount of volume-contraction which takes place during its progress. Since the reaction is the more energetic in proportion as the condensation is greater, it follows that in the case of those elements which exist in different allotropic states, the chemism should be a maximum in those forms whose density is furthest removed from that of the final product, which of course is the same for all the vari-

eties. In the case of sulphur, the rhombic form has a density of 2·07 and the amorphous of 1·92; now the latter is the more readily attacked by nitric acid and permanganate, combines with nascent hydrogen more readily, and is more active in forming trithionic acid, as Berthelot has shown. Amorphous red selenium, according to Schaffgotsch, has a density of 4·26, the vitreous variety a density about 4·28, and the black granular or metallic selenium one of 4·8. But experiment does not show any marked difference between these varieties as regards their chemical activity. In the case of phosphorus, however, the result is well-marked. Common phosphorus has a density of 1·826–1·840, according to Schrötter, while that of the red variety is 2·106 (2·19 as given by Hittorf), and of the crystalline metallic 2·34 (Hittorf). Now ordinary phosphorus combines with oxygen with ignition at 60° and with sulphur at a little below 100°; while red phosphorus requires temperatures of 260° and 230° respectively. The same differences in activity are observed toward other elements, common phosphorus even reducing copper, silver and lead from their solutions. Carbon as diamond has a density of 3·5, while the density of graphite varies from 2·1 to 2·6, and that of amorphous carbon from 1·4 to 2. When treated with a mixture of potassium chlorate and strong nitric acid the diamond is absolutely unattacked, the graphite yields graphitic acid slightly soluble in water, while the amorphous carbons give humus-like bodies and dissolve completely. The relative combustibility of these varieties is proof in the same direction. According to Violette, charcoal prepared at 350° has a density of 1·50 and inflames at 360° to 370°; that made at 432° a density of 1·709, and ignites at 400°; that heated to 1250° a density of 1·86 and an igniting point of 600° to 800°; and that prepared at the fusing point of platinum a density of 2·00, and inflames at 1200°. Similar phenomena are observed with the various forms of silicon, boron, arsenic and tin. In no case does the chemism increase with the density. The author, therefore, concludes that the activity of the chemical process stands in direct relation to the increase in density which takes place during chemical union.—*Liebig's Ann.*, ccxviii, 113, April, 1883. G. F. B.

4. *On the behavior of Nascent Hydrogen in presence of Oxygen gas.*—TRAUBE has given his views on the production of hydrogen peroxide which differ radically from those of Hoppe-Seyler. When, for example, water acts on zinc at ordinary temperatures in presence of oxygen, zinc hydrate (not zinc oxide) is formed together with hydrogen peroxide, according to the equation

$$Zn+\begin{matrix}OHH\\OHH\end{matrix}+O_2=Zn(OH)_2+H_2O_2.$$

Whence Traube concludes that hydrogen peroxide is not an oxidation product of water (water not being directly oxidizable), but a compound of the unbroken oxygen molecule with two detached hydrogen atoms. In further proof of this position he

instances the fact that in the voltaic decomposing cell the hydrogen peroxide never appears at the positive electrode, where the most active oxidation takes place and where the water must be oxidized by the active oxygen were it capable of it, but at the negative electrode where the process is one of reduction and in presence of only passive oxygen. Hydrogen peroxide is therefore a reduction product of the passive oxygen molecule. Moreover, direct experiment shows that nascent hydrogen evolved from zinc and either sulphuric acid or sodium hydrate does not, in presence of oxygen, cause the oxidation of either sulphindigotic acid or of ammonia; and that, as Graham had already observed, hydrogen-palladium gives up none of its hydrogen at ordinary temperatures, even in a vacuum. The oxidizing action of hydrogen-palladium is due to its specific action not to the evolution of nascent hydrogen. Traube therefore denies the assertion of Hoppe–Seyler that nascent hydrogen has the property of rendering oxygen active. True, neutral solution of potassium iodide and starch is blued by palladium-hydrogen and oxygen; but this is due to the fact that hydrogen peroxide is first formed and then this, in presence of the palladium, sets the iodine free. Just as in presence of platinum sponge, hydrogen peroxide blues potassium iodide and starch and guaiacum tincture, and oxidizes nitrite to nitrate, as observed by Schönbein. Traube finds that the peroxide, which alone is indifferent to carbon monoxide, oxidizes it easily in presence of hydrogen peroxide. The oxidation of sulphindigotic acid is similarly accounted for. To show it the author takes seven grams of palladium well charged with hydrogen and agitates it for a quarter of an hour with air and 300^{cc} water made intensely blue with $\frac{1}{1000}$ of potassium sulphindigotate. The blue color decreases but slightly and hydrogen peroxide is formed abundantly. If now the solution be poured off from the metal and allowed to stand with exclusion of air, it becomes gradually decolorized by the action of the H_2O_2, a clear proof that the oxidation is not due to the direct action of the palladium-hydrogen. Traube therefore comes to the conclusion, (1) that palladium-hydrogen does not evolve active hydrogen; (2) that nascent hydrogen does not make oxygen active; (3) that living tissues do not evolve hydrogen, and (4) that the peculiar action of palladium-hydrogen depends upon the formation of hydrogen peroxide, which produces the oxidation, in part directly and in part by the aid of metallic palladium.—*Ber. Berl. Chem. Ges.*, xvi, 1201, May, 1883. G. F. B.

5. *New form of Electrical Accumulator.*—Julius Elster and Hans Geitel show that Zamboni's dry piles can be used as accumulators. The copper pole of the pile is connected with the positive and the tin pole with the negative poles of a Holtz machine. After the latter has been worked for a few minutes the dry pile is found to be charged. After repeated discharges the pile is found to contain a charge of considerable intensity. The authors recommend the following form of pile. The plates of the pile are

strung by means of a needle upon a silk thread and then stretched between the poles of a Holtz machine. A pile of 11,000 pairs of plates of one square centimeter surface, after ten minutes charging, gave shocks one millimeter long and made a Geissler tube luminous. The light of the tube was continuous at first but afterwards became intermittent. Dry piles were also made of one metal. Plates of lead foil were coated on both sides with tissue paper by means of potash water-glass to which a little oxide of lead was added. A pile of 7,000 of the lead plates one square centimeter in section could be charged so as to exhibit strong polarization. A certain amount of moisture must be communicated to the piles. The superoxide of lead deposited electrolytically acts more powerfully than when deposited in any other way. A pile of 1000 plates, coated on one side with chemically produced superoxide and on the other with protoxide of lead, gave proportionally much less tension. These piles are well suited to exhibit to a large audience the principle of Planté's or Faure's accumulator.— *Wiedemann's Annalen*, 1883, No. 7, pp. 489–491. J. T.

6. *Apparatus for the Demonstration of Foucault's Currents.*— Dr. A. VON WALTENHOFEN suspends between the poles of an electro-magnet a pendulum bob which is made in the form of an arc of a circular ring. The pendulum vibrates in a direction perpendicular to the axes of the poles of the electro-magnet. Before the electro-magnet is made the time of swing of the pendulum is noted and compared with the time of swing when the current is passing through the electro-magnet. In the latter case the diminished number of swings can be clearly seen by a large audience. This method is capable of quantitative measurement.— *Wiedemann's Annalen*, 1883, No. 8[b], pp. 928–932. J. T.

7. *Dependence of the Magnetising Function of Steel upon its Hardness.*—HUGO MEYER reviews his numerical results upon this question—the magnetizing function being the magnetic moment divided by the magnetizing force—and considers the physical effects produced by the presence of a hardened layer of steel surrounding a core which is less hardened. He makes various suppositions to reconcile his results with the hypothesis that a piece of steel is made up of small magnets which are turned in their beds when the bar is magnetized. His suppositions are so unlikely that doubt is cast upon the molecular theory of magnetism.— *Wiedemann's Annalen*, 1883, No. 8[b], pp. 849–856. J. T.

8. *Experiments bearing upon the Electro-magnetic Theory of Light.*—In a voluminous paper containing a large series of observations, QUINCKE endeavors to verify Maxwell's conclusion that the square root of the dielectric constant must be equal to the index of refraction for light of the same substance. The several methods adopted give results which are not in accordance with Maxwell's theory of light. Quincke explains the different results obtained by different observers upon the effect of electric force on the index of refraction of certain substances by the following:

(1.) Experiments show a fluctuation in the value of the index of refraction which is due to the electric force.

(2.) A comparatively long duration of the electric pressure causes a fall in the value of the index of refraction equivalent to the effect of a rise of temperature between 0·0001° to 0·1° C. This increases with the difference in potential between the electrodes, and can be attributed to internal friction caused by electric attractions and repulsions between the particles of the fluid.

(3.) The phenomena of the change of the index of refraction show that the electric pressure has no analogy with hydrostatic pressure.

(4.) These changes in the index indicate changes in hydrostatic pressure in the interior of the fluid which are caused by the electrical pressure, the fluid being set into vertical movements thereby.

(5.) The electrical effects appear to be transmitted through the uid by impulses and not in a continuous manner.— *Wiedemann's Annalen*, 1883, No. 8[b], pp. 705–782. J. T.

9. *A method of determining the Ohm.*—In a valuable paper, entitled "On the Determination of the Intensities of powerful Magnetic Fields," by Mr. A. GRAY, which is too technical for a short abstract, the author proposes a method of determining the ohm which is nearly the converse of that of Weber. The method is to hang a coil, of which the constants are known, in a sufficiently intense and uniform magnetic field and find the decrement of the oscillatory motion produced by the induction. The author believes that the calculations and corrections would be, in certain respects, simpler than the method given by Weber.—*Phil. Mag.*, Aug., 1883, pp. 144–146. J. T.

10. *A new Seismometer.*—Professor EWING, of Tokio, Japan, proposes the following form of Seismograph: A common pendulum, having its center of gravity below its point of suspension, is stable. An inverted pendulum with a pivoted supporting rod is unstable. By placing an inverted pendulum below a stable one and connecting the bobs so that any horizontal displacement will be common to both, the system can be made as neutral in regard to stability as one may desire. The instrument has not, however, been tested in an actual earthquake.—*Nature*, July 26, 1883, p. 308. J. T.

11. *Infra red spectra.*—M. HENRY BECQUEREL describes a method of studying the spectra emitted by the vapors of sodium, magnesium, calcium, potassium, silver and thallium which opens a field of observation between the wave-lengths 760 and 1300, an interval greater than that between the extreme red of the visible spectrum and the last ultra violet rays.—*Comptes Rendus*, July 9, 1883. J. T.

II. GEOLOGY AND BOTANY.

1. *Paramorphic Origin of the Hornblende of the Northwestern States;* by R. D. IRVING.—The writer of the note communicated to the August number of this Journal, referring to my paper on the above subject in the July number, says that I seem

to imply therein "that no one except Dr. Wichmann has ever studied microscopically the rocks of the Marquette district, and no one except myself advocated the secondary origin of the hornblende in the 'greenstones' and the eruptive origin of the diabases." Just what greenstones and diabases are here referred to I do not understand, but in reply I have to deny that the paper was ever intended to contain any such implications; nor can it be construed, so far as I can see, to contain them.

The writer of the note also says that I have advocated, without acknowledgment, his previously published views as to the "trachytic and rhyolitic nature of the Keweenaw felsites." To this I have to reply that I have never held the view which he attributes to me. In the Keweenaw series occur, as I was the first to announce, so far as I am aware, original masses, not only of basic but also of acid eruptives, and of eruptives of intermediate acidity, the various kinds constituting a continuous series from the most basic to the most acid. In their general relations, and to a certain extent in their general characters, these rocks correspond to the Tertiary basalts, trachytes and rhyolites, but I have never held the view that any of these ancient rocks are lithologically identical with the Tertiary rhyolites and trachytes, though the words used in the second annual report of the Director of the U. S. Geological Survey might possibly be so construed as to mean this. My acquaintance with the modern eruptives is not yet great enough to warrant the holding of such an opinion against the accepted views of the most eminent lithologists of the day.

What the writer means to imply by his reference to the peridotite of Presq' Isle I do not understand—as I have never published anything, so far as I know, directly or indirectly referring to this rock.

Finally, in reply to the general summing charge of the writer of the note, I have merely to deny that I have ever published as original with myself any of his previously published views. And the truth of my denials will, I am sure, be quite apparent to candid minded persons acquainted with the literature of the subject, to whose judgment I am quite content to leave the matter without further argument.

2. GEORGE VASEY: *The Grasses of the United States; being a Synopsis of the Tribes and Genera, with Descriptions of the Genera and a List of the Species.* (Department of Agriculture; Special Report, No. 63. Washington: Government Printing Office. 1883.)—As Botanist of the Department of Agriculture, as well as by predilection, Dr. Vasey is particularly interested in the *Gramineæ*, and has studied them most assiduously. Availing himself of Mr. Bentham's recent elaboration of the order in the new Genera Plantarum, he has now given, in English, a succinct conspectus of our grasses, characters of the 114 genera known to be North American, and a catalogue of 589 species, indicating their geographical distribution. All this in a pamphlet of forty-seven pages, which is distributed by the Agricultural Department

at Washington, so that it may be in the hands of all our botanists, to whom it will be a seasonable and considerable help. A. G.

3. SERENO WATSON: *Contributions to American Botany. XI.* (Extract from Proceedings of the American Academy of Arts and Sciences, vol. xviii, issued August 15, 1883).—A year ago, in the seventeenth volume of the American Academy's Proceedings, Mr. Watson brought out the tenth part of his important Contributions, half of which was occupied with the determination of the Polypetalæ of Dr. Edward Palmer's collection in northern Mexico and the adjacent part of Texas. This account is completed in the paper now before us, of which it occupies about one hundred pages. The new *Compositæ* are enumerated, but their characters are given in a later-published paper by another hand. In other orders there are a goodly number of new species, some of particular interest.

Omphalodes aliena and *O. cardiophylla* Gray, published in Hemsl. Biol. Centr. Am., are certainly very remarkable, as representing that genus in the New World, as also for the more extended insertion and sometimes plane wings of the nutlets; so that they truly "seem to belong to *Paracaryum* rather than to *Omphalodes*, if these genera are to be retained as characterized by Bentham and Hooker." But, indeed, they were referred to *Omphalodes* on the idea that *Paracaryum* ought not to be so retained. Some notes which, with our original characters, were not printed by Hemsley, were thought to show that certain genuine species of the former genus nearly agreed in the elevated gynobase, etc., with those that have been referred to *Paracaryum*.

Pinus latisquama Engelm., published in the Gardeners' Chronicle with wrong foliage (that of *P. Ayacahuite*) is here correctly characterized.

Spiranthes of Richard is changed on page 159 to *Speiranthes*. This, although six times repeated, is purely accidental, and was by no means intended as a correction of Richard's latinization of the Greek word concerned, which is that of Virgil and Ovid.

Glyphosperma (*Palmeri*) and *Hemiphylacus* (*latifolius*) are two new genera of *Liliaceæ* from Mexico, the first of the subtribe *Anthericeæ*, the second verging to the *Chlorogaleæ*.

Tradescantia leiandra Torr., is declared to belong to that genus, not to *Zebrina*. A note gives a revision of our species of that genus and of *Commelina*.

The *Gramineæ* have been worked out with much pains-taking, and a good beginning made in the study of the grasses of the Mexican border, while awaiting the long-delayed work of Fournier upon those of Mexico. *Bouteloua* is elaborated in a footnote, and twenty-five species recognized.

A few closing pages are devoted to "Descriptions of some New Western Species" of the United States, the most interesting being two new *Microstyles*, discovered by Mr. Lemmon in southwestern Arizona, and *Montia Howellii*, from Oregon, sent by the brothers Howell, with some anomalous features. There are, of course, one or two new *Eriogona* and an *Allium*.

As to Dr. Palmer's collection, not only are his plants nearly all determined, but also those of a considerable collection made by the late Dr. Schaffner, sent in the year 1880 to the present writer, which has been divided between the Gray herbarium and that of Kew. This collection was from the same district (San Luis Potosi) as Parry and Palmer's collection, whose numbers are also cited in considerable part. A small collection sent by Professor Dugès from farther south (Guanajuato) is also reported on. So that these two Contributions — parts x and xi—are of very great importance in the study of the botany of the northern States of Mexico, a district which is becoming more and more open to American explorers. Dr. Palmer's present collections, and those of his forerunner Dr. Gregg, were made in Nuevo Leon and Coahuila. Chihuahua is now equally accessible; and to the western part of that State, and to adjacent Sonora, which may be reached in time, we look for the most interesting future accessions to the botany of the Mexican frontiers. A. G.

4. Professor HERMAN MÜLLER. *The Fertilization of Flowers;* translated and edited by D'ARCY W. THOMPSON, B.A., etc., with a preface by CHARLES DARWIN. With illustrations. London: Macmillan & Co. 1883, pp. 669, 8vo.—The original German edition was of the year 1873, and in this new one, edited as well as translated by Mr. Thompson—turned into good idiomatic English and attractively printed—is incorporated a large mass of the author's recent observations as well as details from his other writings. An added interest is given by the prefatory notice, "full of suggestions, full of kindly, appreciative feeling," which is one of the very last issues of Mr. Darwin's pen. A sadder interest is now given to the volume by the announcement which has just reached us of the death of Dr. Müller, on the 25th of August last. A great, and we judge a premature loss, for though we believe he was not young, he cannot have been old. His earliest published writings of which we have record do not date back of 1850, and the first upon the subject which he had made so fully his own has the date of 1869. Meanwhile, by his most laborious and patient observations, by his great acuteness in interpretation and research, and by his studies of the modifications of insects in relation to flowers, no less than those of flowers to insects, he had placed himself at the head of this curious branch of biology, which was initiated by his countryman, Christian Conrad Sprengel about one hundred years ago, resuscitated and more broadly based in our days upon "the Knight-Darwin law," and the lead in which, since Darwin's death, is restored to Germany mainly by the researches of Hildebrand and Herman Müller.

At this moment there is neither time nor room for more than the announcement of this volume, which forms an encyclopedic text-book upon this subject, and which will be largely used as such in this country, where the way is well prepared for it. The book has full indexes, both of plants and insects concerned, and a full bibliography of 814 entries, filling 32 pages. There are

many things upon which one would wish to remark, and there is one little omission. "The observations of Darwin (1861), and Asa Gray (1862)," are referred to through the bibliography, but without noting that the first did not anticipate, and that the second did anticipate and announce in this Journal, Müller's later discovery of the mode of fertilization of *Cypripedium*, in 1868, which he describes on page 339. The curious modification of the papilionaceous flower in *Apios* is unnoticed. A. G.

III. Miscellaneous Scientific Intelligence.

1. *American Association for the Advancement of Science at Minneapolis.*—The meeting of the Association at Minneapolis, under the presidency of Professor C. A. Young, of Princeton, closed on Wednesday, August 22d. The number of members present was large, but less than at Montreal and Boston, for the obvious reason that the place was less central for workers in science. They were most generously treated by the citizens of Minneapolis.

The sections which had the largest number of papers were the geological and archæological, and the geological subject which absorbed the most time and attention was that of the phenomena of the Glacial era and later Quaternary in America.

Dr. J. W. Dawson, in his address as retiring president, "On some Unsolved Problems in Geology," spoke first of some of the peculiarities and divisions of the Archæan rocks, taking, in his few words, the side of the probable pre-Cambrian age of the Taconic system, and adding beyond, "after the solitary appearance of Eozoon in the Laurentian, and of a few uncertain forms in the Huronian and Taconian, we find ourselves in the Cambrian," etc. Dr. Dawson next spoke briefly of the bearing of geological facts on the theory of evolution, of certain new views as to the origin of coal from cryptogamous vegetation, and of the events of the Glacial era.

The "unsolved problems" are treated in the address without much labored argument or much effort to solve them beyond what is contained in the expression of an opinion. For example, on the Taconic question the remark about "uncertain forms" or fossils in its beds, is the best fact on the subject stated, there being no reference to the recent investigations of the rocks of the Taconic Range, and their stratigraphical relations to associated beds containing fossils that are not "uncertain," found both in Vermont and in Dutchess County, New York, or the good fossils in the Poughkeepsie Taconic slates.

The address of Professor Rowland, vice-President of the Chemical Section, was an admirable "plea for true science." One sentiment, not new, but true, is here cited to aid in its dissemination. He says, with reference to the government duty on foreign books and periodicals, "We call this a free country and yet it is the only one in which there is a direct tax on the pursuit of science. The

low state of pure science in the country may possibly be attributed to the youth of the country; but a direct tax to prevent the growth of our country in that subject cannot be looked upon as other than a deep disgrace." In the science of physics "no books above elementary ones have ever been published, or are likely to be published, in this country; and yet every teacher in physics must have them, not only in the college library but on his own shelves, and must pay the government of this country to allow him to use a portion of his small salary to buy that which is to do good to the whole country." "One would think that books in foreign languages might be admitted free; but to please the half-dozen or so workmen who reprint German books, not scientific, our free intercourse with that country is cut off."

Professor W. A. Rogers, vice-President of the Section of Mathematics and Astronomy, treated in his able address of the German survey of the Northern Heavens.

The address of the vice-President of the Geological Section, Professor C. H. Hitchcock, was on "the Early History of the North American Continent." Professor Hitchcock presented views with reference to the origin and subdivisions of the early crystalline rocks, and the occurrence of many "oval" isolated Archæan areas in eastern North America, and put forth his latest conclusions as to the *igneous* derivation of the Laurentian areas, arrived at after a visit to the volcanic Hawaiian Islands, he holding, in other words, that "the first land consisted of volcanic islands."

A lecture was delivered also by Professor E. D. Cope before the association in general session, on the evidence for evolution in the history of the extinct mammalia. It was a valuable discourse by one who had, to a very large extent, gathered his own facts. He observes that since 1860 the number of known American species of mammals has increased from 250 to nearly 2000.

These addresses are published in full in Science, numbers 27 to 31.

On the subject of the Quaternary the following are some of the facts presented.

Professor G. F. Wright treated of the southern limit of the glacier, giving, first, credit to earlier investigators, and speaking of Professor W. Upham as the first to survey the whole of the part of the line which lies between Cape Cod and Brooklyn, and Professors Cook and Smock, that across New Jersey. The historical facts presented are given in this Journal in various notices and in papers by Mr. Upham, Professor Smock, Professor Wright, Professor Chamberlin and others. Professor Wright added the following to the account of the southern limit in his recent paper; that the line through Indiana grazes again the edge of Kentucky opposite Madison, and reaches its southernmost point near Charleston; then bears north through Scott and Bartholomew Counties and follows the line between the latter and Brown County to the northeast corner of Brown; then turns southwest

to the northeast corner of Monroe; then bears northward for ten miles to near Martinsville, in Morgan County; then passes west and south, diagonally, through Owen and Green Counties, and to Knox in Harrison township, in Knox County.

Professor CHAMBERLIN, who also early surveyed the line from Long Island westward, presented an important paper on "the Terminal moraines of the second Glacial epoch."

Professor I. C. WHITE treated of the high terraces of western Pennsylvania, a subject discussed by Professor J. J. Stevenson in this Journal for 1878 (III, xv, 245), and said that the facts were wholly in accordance with the view brought out by Professor Wright as to the existence, in the Glacial era, of an ice-dam 500 to 600 feet high across the Ohio valley at Cincinnati. The five several terraces at Morgantown are 30, 75, 175, 200 and 275 feet above the river. The deposits of the upper terrace "consist almost entirely of clays and fine sandy material, with few bowlders." The terrace and its clays have been traced, at the "same absolute level," on other tributaries of the Monongahela, namely, Decker's, Dunkard, Whitely, Muddy and Ten Mile Creeks. The dam set back the waters of the Kanawha and filled the Mud-Guyandotte arm of the valley; and afterward its removal left the valley high and dry. These and other facts were regarded as proving the truth of Professor Wright's hypothesis. The height of dam at Cincinnati required by the facts, according to Professor White, is about 625 feet, or 1050 to 1075 above tide-level.

Professor W. UPHAM presented a paper on the Minnesota valley in the Ice Age in which he gave his reasons for holding that the existence of the great flooded Winnipeg lake — Lake Agassiz, as he calls it—was due to a damming of the waters of the region by a barrier of glacier ice on its north margin preventing the waters from flowing into Hudson's bay, their present course. He appeals to two epochs of glaciation, and remarks that in the first the area was covered with drift, mostly the unstratified; during the interval between the two the pre-glacial valley of the region was deepened by the drainage; during the later Glacial epoch the till only partially blocked up the river-course along its greater part, and portions that were obstructed were channeled anew from the mouth of the Minnesota to New Ulm. As long as the ice-barrier existed on the north the waters poured from "Lake Agassiz" down the Minnesota to the Mississippi, at first depositing drift, but later, when the waters carried little drift material, excavating the existing river-channel through the deposited drift. This view makes the deposits of the valley to have been formed in one era and the channel, between the terraces, in another following. He holds also that the water of Lake Superior may have been held back in a similar way, at a level about 500 feet above its present height, and explains certain facts connected with the St. Croix River on this supposition.

Another paper by Professor Upham treated of the "Changes in the currents of the ice of the last Glacial epoch in eastern Minnesota."

Dr. NEWBERRY presented a paper "On the eroding power of ice," attributing to it far greater effects than are conceded by most writers on the subject, and making it, as he had done before, the excavating agent of the basins of the Great Lakes. He also spoke, with a good array of facts, on the reality of the age of ice, and the wide extension of the great northern glacier.

A paper by Professor G. H. STONE, "On the Kame rivers of Maine," was read by Professor Upham, in which the origin of kame deposits was discussed, whether super-glacial or sub-glacial, and favoring the former view. He made the important statement that in Maine no southward motion in the glacier would have been possible after the ice had thinned down to 500 feet on account of transverse ranges of hills.

Professor Upham read a paper by Miss F. E. BABBITT, of Minnesota, "On Glacial Man in Central Minnesota." The evidence consists in the discovery of "rudely worked quartzes." They were first found by Professor N. H. Winchell in the top portion of certain terraces in Central Minnesota about 100 miles northwest of St. Paul, in the village of Little Falls, Morrison County. Recently similar discoveries have been made in the same region by Miss Babbitt in beds of the terraces below the upper. Many hundreds of chips were found in a small area, none worn, although the stony material of the terrace was well rounded; and every freshet turned out other specimens. The specimens come from a well limited layer, a few inches thick, 12 to 15 feet below the top of the terrace.

In the discussion which followed the reading of the paper, Professor F. W. Putnam stated that some of the specimens exhibited in illustration of the paper were unquestionably of human workmanship.

Other papers on related subjects are those of W. J. MCGEE, "On the formation of Glacial Cañons"; of JULIUS POHLMAN, "On the history of the Niagara River"; of W. MCADAMS, "On the læss and glacial clays of Alton, Illinois.

The following is a list of the papers accepted for reading at the sessions of the Association:

List of Papers accepted for Reading.

Section A, Mathematics and Astronomy.

EDWARD S. HOLDEN: The total solar eclipse of May 6, 1883.

W. A. ROGERS: A new method of investigating the flexure corrections of a meridian circle; On an improved method of producing a dark-field illumination of lines ruled upon glass.

E. W. HYDE: The calculus of direction and position.

S. C. CHANDLER, JR.: Results of tests with the almacantar in time and latitude; Investigations of light variations of Sawyer's variables.

J. B. EASTMAN: Internal contacts in transits of inferior planets.

G. W. HOUGH; Physical phenomena on the planet Jupiter; The rotation of domes.

J. JANSSEN: French observations of the total eclipse of May 6th, 1883.

O. S. WESTCOTT: Some hitherto undeveloped properties of squares.

E. F. SAWYER: On the light variations of *T. Monocerotis*.

EDGAR FRISBIE: Orbit of the great comet of 1882.

J. K. REES: Observations on the transit of Venus made at Columbia College, New York City: Description of new observatory of Columbia College.

C. M. WOODWARD: Descriptive geometry applied to the general ellipsoid.

J. BURKITT WEBB: Conic sections in descriptive geometry; Descriptive geometrical treatment of surfaces of the second degree; The kinematics of rotary pumps.

C. A. YOUNG: Some observations on Uranus.

D. P. TODD: Methods of observing eclipses of Jupiter's satellites.

SAMUEL EMERSON: Standard time-pointer and a time longitude dial; System of algebraic geometry.

J. M. BATCHELDER: Tidal observations on soundings distant from shore.

Section B, Physics.

W. A. ROGERS: Definitive determination of the relation: Imperial yard $+3{\cdot}37030$=the meter of the archives.

E. S. MORSE: On the utilization of the Sun's rays for warming and ventilating apartments.

H. S. CARHART: The magnetophone, or the modification of the magnetic field by the rotation of a perforated metallic disc.

A. E. DOLBEAR: The static telephone.

F. E. NIPHER: Magnetic survey of Missouri; Plan for a state weather service.

T. C. MENDENHALL: A method of distributing weather forecasts by means of railways.

P. R. HOYT: The tornado at Racine, May 18, 1883.

J. H. FRICK: The tornado of May 18th.

C. E. FRITTS: On a new form of selenium cell, and some remarkable electrical discoveries made by its use.

H. T. EDDY: On the kinetic theory of the specific heat of solids; a kinetic theory of melting and boiling; An extension of the theorem of the virial to rotary oscillation.

B. F. THOMAS: A method for the calibration of a galvanometer; A method of determining the center of gravity of a mass; Two forms of apparatus for Boyle's law; A new heliostat.

SAMUEL HART: Natural snow-balls or snow rollers.

GUSTAVUS HINRICHS: Remarks on the tracings of self-registering instruments and the value of the S. S. indications for Iowa in June and July, 1883.

Section C, Chemistry.

R. B. WARDER: Suggestions for computing the speed of chemical reactions.

J. W. LANGLEY and C. K. MCGEE: On the sub-aqueous dissociation of certain salts.

C. F. MABERY and H. H. NICHOLSON: On gamma-dichlordibromopropionic and gamma-dichlorbromacrylic acids.

C. F. MABERY and RACHEL LLOYD: On the formation and constitution of chlordibromacrylic acid.

C. F. MABERY and G. M. PALMER: On orthoiodtoluolsulfonic acid.

H. W. WILEY: American butters and their adulterations.

CLIFFORD RICHARDSON: The composition of American wheat and corn; Sotol, Mexican forage plant.

E. L. STURTEVANT: Twelve months of lysimeter record at the New York Agricultural Experiment Station.

W. H. SEAMEN: New forms of burettes.

C. LEO MEES: Estimation of carbon and nitrogen in organic compounds.

Section D, Mechanical Science.

W. A. ROGERS: A method of testing long plane surfaces, applicable to the alignment of planer-beds, lathe-beds, heavy shafting, etc.

R. H. THURSTON: The commercial and dynamic efficiencies of the steam-engine; Centrifugal action in turbines.

T. R. BAKER: A comparison of terra-cotta lumber with some other building materials.

C. M. WOODWARD: Descriptive geometry applied to the general ellipsoid; Velocity of the piston of a crank engine.

J. B. WEBB: Improvements in shaping machines; Regularity of flow in double-cylinder rotary pumps.

Section E, Geology and Geography.

G. F. WRIGHT: Result of explorations of the glacial boundary between New Jersey and Illinois.

T. C. CHAMBERLIN: The terminal moraine west of Ohio.

WARREN UPHAM: The Minnesota valley in the ice age; Changes in the currents of the ice of the last Glacial epoch in eastern Minnesota.

I. C. WHITE: Relation of the glacial dam at Cincinnati to the terraces on the Upper Ohio and its tributaries.

N. H. WINCHELL: The comparative strength of Minnesota and New England granites; Clay pebbles, with an exhibition of specimens from Princetown, Minnesota.

J. W. DAWSON: On Rhizocarps in the Paleozoic period.

JAMES HALL: On the microscopic structure of the test of fossil Brachiopoda.

W. J. MCGEE: On Glacial canyons.

JULIUS POHLMAN: The life-history of the Niagara River.

H. C. BOLTON and A. A. JULIEN: The singing beach of Manchester, Mass.

A. S. TIFFANY: The equivalent of the New York water-lime group developed in Iowa.

RICHARD OWEN: The Earth's orographic framework: its seismology and geology; The "Continental Type" or normal orography and geology of continents.

J. W. CHICKERING, Jr.: Thermal belts.

WM. MCADAMS: A new vertebrate from the St. Louis limestone; Animal remains from the Lœss and Glacial clays.

G. H. STONE: The kame rivers of Maine.

J. D. DANA: Evidences from southern New England against the iceberg theory of the drift.

J. S. NEWBERRY: On the eroding power of ice; The ancient glaciation of North America, its extent, character and teachings; On the genesis and classification of mineral veins.

T. STERRY HUNT: The Pre-Cambrian rocks of the Alps; On the serpentine of Staten Island, New York.

JAMES MACFARLANE: The "earthquake" at New Madrid, Mo., in 1811, probably not an earthquake.

E. W. CLAYPOLE: On the Hamilton sandstone of middle Pennsylvania; On a large Crustacean from the Catskill group of Pennsylvania; On Rensselaeria and a fossil fish from the Hamilton group of Pennsylvania.

E. D. COPE: On the structure of the skull in Diclonius mirabilis, a Laramie Dinosaurian; On the Tuberculate type of superior molar and the origin of the Quadritubercuiate; On two primitive types of Ungulata.

WM. MCADAMS: Animal remains from the Lœss and Glacial clays.

A. A. JULIEN and H. C. BOLTON: The singing beach of Manchester, Mass.

G. F. KUNZ: Topaz and associated minerals from Stoneham, Oxford county, Maine; Colored tourmalines and lepidolite crystals from a new American locality; A note on the finding of two American beryls; Andalusite from a new American locality; On a white garnet from near Hull, Canada.

Section F, Biology.

W. G. FARLOW: Relations of certain forms of Algæ to disagreeable tastes and odors; The spread of epidemic diseases in plants.

D. S. KELLICOTT: Psephenus Lecontei; on the external anatomy of the larva.

E. L. STURTEVANT: Parallelism of structure of maize and sorghum kernels; Influence of position on seed; Agricultural botany.

D. P. PENHALLOW: Relation of root and leaf areas; Corn.

E. W. CLAYPOLE: Note on the present condition of the box huckleberry, Vaccinium brachycerum, in Perry county, Pennsylvania; Notes on the potato beetle and Hessian fly for 1883.

C. P. HART: Conscious automatism.

E. S. MORSE: Mya arenaria; its changes in Pliocene and prehistoric times; On a new plan of museum case.

S. H. GAGE: Pharyngeal respiration in the soft-shelled turtule, Aspidonectes spinifer.

W. R. DUDLEY: An abnormal orchid, Habenaria hyperborea; Origin of the Flora of the central New York lake region.

MISS M. E. MURTFELDT: Periodicity of Sabbacia angularis.

E. P. HOWLAND: The application of nitrous oxide and air to produce Anæsthesia, with clinics on animals in an experimental air chamber.

J. M. COULTER: Development of a dandelion flower.

W. J. BEAL: Leaves of the Gramineæ with closed sheaths.

J. C. ARTHUR: A supposed poisonous sea-weed in the lakes of Minnesota.

JOSEPH F. JAMES: The position of the Compositæ in the natural system.

B. G. WILDER and S. H. GAGE: On the use of vaseline to prevent the loss of alcohol from specimen jars.

HERBERT OSBORN: Note on Phytoptidæ.

E. S. BASTIN: A fact bearing upon the evolution of the genus Cypripedium.

C. V. RILEY: The Psyllidæ of the United States; Some recent discoveries in reference to Phylloxera.

A. HYATT: Observations on Cephalopoda.

Section H, Anthropology.

F. W. PUTNAM: An abnormal human skull from a stone grave in Tennessee; A new stand for mounting skulls, by E. E. Chick.

J. O. DORSEY: Osage war customs.

MRS. ERMINNIE A. SMITH: Accidents or mode sides of verbs in the Iroquois dialects; Life among the Mohawks in the Catholic missions of Quebec Province; Studies in the Iroquois concerning the verb *to be* and its substitutes.

S. D. PEET: The correspondence between the prehistoric map of North America and the system of social development; Game drives among the emblematic mounds; Typical shapes among emblematic mounds: the different attitudes exhibited by the same animal; High places connected with ancient villages; the religious structures common to villages in prehistoric times; An ancient village of the emblematic Mound Builders; Caches guarded by effigies; Effigies guarding the village, and sacrificial places not far away.

O. T. MASON: The Charnay collection at Washington.

E. S. MORSE: Kitchens of the East; Methods of arrow release; In-door games of the Japanese.

WM. MCADAMS: The great mounds of Cahokia.

CHARLES WHITTLESEY: Metrical standards of the Mound Builders by the method of even divisors.

JOHN CAMPBELL: The Mound Builders identified.

E. P. WEST: Personal observations of the Missouri River mounds from Omaha to St. Louis considered from a geological stand-point. Their invariable association with the Lœss and Terrace formation.

MISS A. C. FLETCHER: Some observations on the laws and privileges of the Gens in Indian society; Symbolic earth formation.

MISS F. E. BABBITT: Vestiges of Glacial man in central Minnesota.

J. W. POWELL: A classification of the sciences.

Section I, Economic Science and Statistics.

ELIZUR WRIGHT: Life insurance and self-insurance.

C. S. MIXTER: The increase of the colored population of the United States.

H. C. HOVEY: Oyster farming in Connecticut waters, with a map of the grounds. specimens of shells, etc.

C. W. SMILEY: The German carp and its introduction into the United States.
E. T. COX: Cable cars for city passenger traffic.
EDGAR FRISBIE: Building associations.
STEPHEN S. HAIGHT: Health foods.
C. V. RILEY: Improved methods of spraying fruit and shade trees for protection against leaf-feeding insects.
J. R. DODGE: Enhancement of values in agriculture by reason of non-agricultural population.
T. E. JEFFERSON: A new system for the treatment of sewer gas.

The next meeting of the Association will be held in Philadelphia, Professor J. P. LESLEY was elected president for the year. The vice-presidents appointed are, Professor H. T. EDDY, of the Section of Mathematics and Astronomy (A); JOHN TROWBRIDGE, Cambridge, Mass., of Physics (B); J. W. LANGLEY, Ann Arbor, of Chemistry (C); R. H. THURSTON, Hoboken, of Mechanical Science (D); N. H. WINCHELL, of Geology and Geography (E); E. D. COPE, of Biology (F); T. G. WORMLEY, of Histology and Microscopy (G); E. S. MORSE, of Anthropology (H); Hon. JOHN EATON, Washington, D. C., of Economic Science and Statistics. Dr. ALFRED SPRINGER, of Cincinnati, was chosen General Secretary, and F. W. PUTNAM was continued as Permanent Secretary.

2. *British Association.*—The fifty-third annual meeting of the British Association was held at Southport, between the 19th and 27th of September.

OBITUARY.

Professor J. REINHARD BLUM of Heidelberg, for many years one of the leading mineralogists of Germany, died on the 22d of August last in his 81st year. Professor Blum's most important contribution to science was his treatise on Pseudomorphs published in 1843, with the four supplements bearing the dates of 1847, 1852, 1863, 1879. This subject he developed with great care and thoroughness, and although subsequent study will doubtless carry the subject much further, the works of Blum will never lose their value and the large number of facts he brought together will remain the foundation of future work in this field. In addition to the publication of the volumes mentioned, he also made a large typical collection of pseudomorphs illustrating the subject described; this collection became in 1870 the property of Yale College and is preserved in New Haven. Professor Blum was also the author of a general work on Mineralogy, and of another on Lithology and of various shorter mineralogical papers. He was a man of most genial and friendly character and his many pupils who listened to his teaching between 1828 and 1877, remember with pleasure the instruction and encouragement which they received from him.

WILLIAM A. NORTON, Professor of Civil Engineering in the Sheffield Scientific School of Yale College, and the author of various papers in this Journal, and of works on Astronomy and Physics, died on the 21st of September, in his 73d year. A fuller notice is deferred to another number.